PROCEEDINGS

OF THE

American Society of Microscopists.

THIRTEENTH ANNUAL MEETING,

HELD AT

DETROIT, MICH., AUG. 12, 13, 14, AND 15, 1890.

VOLUME XII.

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ERRATA.

Page 96, lines 1 and 4, for "Englypha," read Euglypha.

Page 124, line 3 from bottom, for "Whitall and Tatam," read Whitall and Tatum.

Page 165, foot-note, for "Aganoid," read A. ganoid.

Page 243, line 29, for "page 247," read page 250.

PROCEEDINGS
OF THE
American Society of Microscopists.

THIRTEENTH ANNUAL MEETING.

ADDRESS OF THE PRESIDENT:
THE INFLUENCE OF ELECTRICITY ON PROTOPLASM.

GEORGE E. FELL, M. D., F. R. M. S., Buffalo, N. Y.

In glancing over the themes presented by the past presidents of this society at the annual meetings over which they have been called to preside, it is noticeable that they include a wide variety of subjects. This could not well be otherwise, when the vastness of the field in which the microscope is an indispensable factor is considered. Blot vision (which the microscope so wonderfully enlarges) from the functions, and what is left to be appreciated? This instrument, which gives its name to our association, is the single factor which teaches us beyond the power of words to delineate how circumscribed is the cycle of nature which the unaided sense of sight may compass. Hence it is not strange that in the presidential addresses of past years themes of the specialist in microscopic investigation, those historical, with others of general interest on the subject, should be presented. From this I assume that no rule is prescribed to guide us in the selection of a topic for your consideration. It appears to be acknowledged in the introduction to several of these discourses that the subject should be made somewhat popular, in deference, no doubt, to the ladies and gentlemen, not specialists, who are interested enough in our labor to grace us with their presence.

In the selection of an address entitled *The Influence of Electricity* *on Protoplasm,* it may be urged that the presentation of a popular paper with such indefinite topics for the subject is almost a hopeless endeavor. Had I other than an American audience, qualified by our advanced educational methods to appreciate intelligently discourses on almost any subject, I might have chosen one less technical in character.

What in part has called the attention of the world to the influence existing between these remarkable agents is that in the medical world, at home and abroad, an awakening interest in the therapy of electricity has lately been aroused; unusual activity prevails; electricity is utilized more frequently than ever before in this field, and a more systematic method of application is in vogue. Another factor also is the controversy, even now in progress, relating to the application of electricity in the execution of criminals. How death is produced by powerful electric influences, from a technically scientific standpoint, is as yet unsettled. It is reasonably anticipated that the investigations by many workers, lately undertaken, may result in clearing up the existing uncertainties, and that the true and scientific value of this wonderful agent in these fields may be definitely ascertained.

Furthermore, the arts and sciences utilize electricity to such an extent that the world looks on in amazement, awaiting eagerly the next electric stride in the amelioration of the conditions which now beset mankind. We are fast approaching the electric age of history, but stand awed in an attempt to contemplate its possibilities.

We will first consider protoplasm as it exists in nature and its associations in the animal organism with reference to the question of electrical conductivity. Demonstrating to my class the circulation of the blood in the web of the frog's foot, the field of vision presented the capillaries with the intermediate substance, composed of numerous variously formed cell-like bodies constituting the tissues of the foot. Histological methods demonstrate that muscle, nerve, adipose, connective or other tissue illustrated in the web which we examined is alike composed of cells or units of protoplasm, vital tissue being therefore a mass of protoplasm.

Protoplasm exists only under certain conditions of environment; it is continually undergoing change (metabolism); its very existence depends upon its property of utilizing certain nutrient substances provided by various means for sustenance, and at the same time the effete products of its vitality must be removed.

We thus have assimilation and disassimilation as conditions which are associated with every protoplasmic particle, from the unicellular ameboid forms to the multicellular and more complex organisms of the mammalian type. This condition prevails, viz., that protoplasm, whether in the simpler or more complex organisms, receives its pabulum from its immediate environment. The living cell, vegetable or animal, is surrounded with a medium which provides material for assimilation. In the lower forms of life the micro-organism—bacterium, micrococcus, or spore—in aqueous surroundings, secures the oxygen necessary to its existence from the fluid in which it moves. In the higher organisms the circulatory fluid provides a similar condition, supplying to each cell its pabulum, enabling it to fulfill its function as well as retain its vital principle or condition. The microscope enables us to study the morphology of the cell and tissue; to note the varying aspects from the nucleolus to nucleus, to the variously formed tissues of the complete organism. Illustrations of this truth might be represented by the admirable study of the nucleolus and modifications in the living cell by Dr. W. H. Dallinger (Proceedings Royal Microscopical Society), where, in the varying vacuolar manifestations and movements of protoplasmic granules, he demonstrated that the *extra vital* substance was associated with the nucleolus. Another instance would be the work recorded by the embryologist, wherein the manifestations of vital changes from the embryo to various tissues and growth to organs and the fully developed organism is known to the minutest detail, and yet we must deplore that the *vital principle,* the immediate and present cause of all of these changes, is beyond the power of the microscope to reveal.

The microscope might possibly indicate a slightly hyaline appearance to the living cell not present after life has departed, produced by the coagulated state of the protoplasmic mass. Herein lies one of the difficulties met with in the investigation of our subject. Many reports indicate that in the death of animals (death of protoplasm) by powerful electric influence no modification of the tissue is discernible with the microscope.

The composition of the tissue of the body has a marked bearing on the subject we are reviewing. As we are considering the relation of electricity to animal protoplasm, we will take up the more important substances relating to the composition of the tissues making the great bulk of the animal organism. That of greatest fluidity, the blood, has of water about 75 per cent.; a fair percentage of salts, chiefly components of potash and phosphoric acid (C. Schmidt). Muscular tissue, which forms a very large proportion of the bulk of the organism, has a jelly-like consistence.

Yeo says, "the contractile substance of muscle is so soft as to deserve the name fluid rather than solid; it will not drop as a liqüid, but its separated parts will flow together again like a half-melted jelly. In this respect it resembles the protoplasm of elementary organisms, the buds from which are so soft that they can unite around foreign bodies, and yet have sufficient consistence to distinguish them from fluid."

The capillaries in the muscle form a fine net-work, which in the active state of the muscle simulates somewhat that condition of congestion produced by the passage of an electric current through living muscle, thus presenting an even greater state of fluidity to this tissue under such conditions. It must be remembered also that all tissue is sustained to a greater or less extent by a circulatory system, and that, even among the more solid portions of the organism, bone has as much as 23 per cent. of water entering into its composition, combined with the salts of the blood circulating in the canaliculi traversing it. The influence of adipose tissue and the epidermis, factors mentioned (Kemmler inquiry) as presenting a marked resistance to the passage of the electric current, must be considered in this connection.

The fat of the body as intermingled with the various tissues of the body is also nourished by a vascular blood supply, and in very few instances (except, possibly, abnormal states) does it present an unbroken barrier to the electric force. In the epidermis, where the greatest resistance is offered, the vascularity produced by the sudoriparous (sweat) ducts, without taking into consideration the capillaries of the papillary derma, would, it seems, be sufficient to eliminate any uncertainty. Furthermore, it is well to note that the animal organism presents a somewhat compact mass, with the exception of the intestinal canal and thoracic cavity in expiration; that outside of this there is practically no break of continuity to the passage of an electric current.

From these generalizations and the statement that fluids with the potassium and sodium salts are better conductors than pure water, it is reasonable to assume that the animal organism, through its great vascularity and chemical composition, presents a medium that may be looked upon as a fairly good conductor of electricity; its real value as a conductor will be considered later. After reviewing a few facts regarding the physics of electricity we will tersely consider the *influence of this agent upon the lower forms of life;* the *chemical or electrolytic changes produced in tissue;* the *conductivity or resistance to the* *passage of the electric force;* the *influence of the electric current on sensation,* and finally the *production of death and the utilization of electricity in the execution of the death penalty.*

It may be noted that investigation of these subjects cannot well be carried on without utilizing the microscope. The unfamiliarity of many with the nomenclature employed in the discussion of subjects pertaining to electricity requires a short explanation regarding the terms most frequently used. We assume that all are familiar with the methods of electro-decomposition illustrated practically in electro-plating. The compound fluid, termed an *electrolyte,* being traversed by the electric current of sufficient strength to overcome the chemical affinity existing between its molecules, is broken up into a more elementary state, and the metals, the salts of which were present in the fluid, are deposited at the electrodes. The effect is termed *electrolysis.* That electrode connected with the positive pole is the *anode;* that with the negative pole the *cathode.* Electro-positive elements appear at the negative, and the electro-negative elements at the positive electrode.

Electro-motive force is the power produced by the electrical generator, be it a galvanic cell, a voltaic pile, or dynamo. The electro-motive force of a Daniell standard cell is one *volt,* the electro-motor unit of measurement. In a standard Daniell cell there is a uniform internal resistance to the transmission of the electro-motive force, which is termed an *ohm,* and is the unit of resistance to the passage of the current.

The electrical current passing through a standard Daniell cell has an electro-motive force of one *volt,* and is termed an ampere. Ohm's law is presented by the equation $C = \frac{E}{R}$, or the strength of the current is equal to the electro-motive force divided by the resistance. From this it is seen that the strength of the current passing through an electrolyte, be it a portion of the human body or a decomposable liquid, as in electro-plating, is measured in amperes or fractions of an ampere; the force of the currents in *volts,* and the resistance of the electrolyte to the passage of the current in ohms. In medical application of electricity the current strength is measured in one-one-thousandths of an ampere or milliamperes, the sensory nervous system being incapable of withstanding strong currents. The milliampere-meter is considered an essential feature of the electro-therapeutical armamentarium, as with it the effects of weak or strong currents may be observed and systematically noted.

The Influence of Electricity Upon the Lower Forms of Life.

Slight electric shocks from a coil (induced current) increase the rapidity of the protoplasmic movements; stronger ones cause tetanic contraction, and numerous and powerful ones produce coagulation. "A constant current causes contraction and imperfect tetanus; and if powerful and long kept up, the positive pole produces in the amœbæ near it the same changes as dilute hydrochloric acid, and the negative pole the same changes as are produced by an alkali, such as potash." Upon infusoria weak electrical currents first quicken the ciliary motion and cause movements of rotation; then produce swelling of the protoplasm, with slower movements, and finally apparent solution of the protoplasm. Moderate currents produce a tetanic contraction of the protoplasm and of the cilia, while the contractile vesicle is unaffected. Strong currents cause liquefaction of the protoplasm. * * * (Pharmacology Therapeutics and Materia Medica, Lauder Brunton, 1885.)

Dr. Klein, in the "Hand Book for the Physiological Laboratory," gives an interesting account of the action of electricity on blood. He explains the method of placing the blood on a slide provided with two poles, when, the cover glass being placed on the slide, the examination is made. According to "Rollet," it is advisable in using electrical discharges that the tinfoil poles should be six millimeters apart. The Leyden jar should have a surface of 500 square centimeters, and give a spark one millimeter long. If, then, the discharges succeed each other at intervals of from three to five minutes the following changes are observed in the colored corpuscles of man: First, the circular discs become slightly crenate. This effect gradually increases; the corpuscles become rosette-shaped, then mulberry-shaped, and, finally, by the acumination of the projections—horse-chesnut-shaped. Later the processes are withdrawn, the blood corpuscles become round and at last pale. The effect on the white blood corpuscles during their movements is to cause them to assume the spheroidal form, but they resume their movement as soon as the current, if not too strong, is discontinued. Under the influence of successive shocks of greater intensity they swell out, their granules exhibiting molecular movement, and finally disappear. Without further consideration of the influence of the electric current upon the lower forms of life, it may be seen that the influence is positive in its character; that we have with the weaker currents an undoubted electrolysis produced, and with the stronger or a long-continued influence of the current a lowered vitality of the protoplasm.

The blood taken by myself from the temple of William Kemmler, the first man executed by electricity, seven minutes after the current had been turned off, presented marked peculiarities. Fully one-third of the field in many instances exhibited corpuscles ranging from a size approximating the normal to the size of ordinary granules. Protrusion of the protoplasm from the corpuscles was frequently noted. (See Plate.)

The Chemical or Electrolytical Changes in Tissue.

In considering this subject we will omit the consideration of *electrotonous*, a condition produced in nervous and adjacent tissue by the passage of an electric current through it, and which has been quite fully considered by physiologists.

As to the influence of the faradic current (alternating current) upon an electrolyte, testimony seems to favor the view that electrolysis is produced, although not in the same degree as with the galvanic current. (Foot-note, "London Telegraph Journal," "Electrolysis by alternating current of dynamo machines.")

The galvanic current will produce the following action: At the positive pole hydrogen gas will be generated; at the negative, an alkalinity in the fluid will be produced. The blood, filling a vascular growth, if a sufficiently powerful current be used, will gradually coagulate. Just what the nature of the effect of the continuous current upon the internodal fluids and tissues of the body may be, it is difficult to state, but the opinion of leading medical electricians is almost unanimous in favor of a change taking place. Amory, in his work on Electrolysis (page 127), says: "There are four methods by which electricity can be supposed to interfere with interstitial nutrition, and, in consequence of the interference, destroy the life of the cells, viz: 1st. By producing a true decomposition of the chemical compounds, upon whose combination the integrity of the living structure depends. 2nd. By interfering with the natural processes of cell segmentation, by which their proliferation and increase is effected; this interference would thus prevent the repair and multiplication of the cells, whose living functions are essential to the growth of the living tissues. 3rd. By promoting a movement of the mass of fluid in the living tissues towards the negative electrode, and thus interfering with the constructive metabolisms upon which interstitial nutrition depends. 4th. The acid and alkaline reactions at the positive and negative electrodes respectively, from which a caustic action upon the tissues is effected through contact of these two different chemical reactions.'' Amory in these statements is endeavoring to find a rational cause for the destruction of abnormal growths.

As to special modification of tissue, in a discussion held before the New York Academy of Medicine November 27, 1889, Dr. A. H. Buchmaster recited the following experiments: The heart of an anæsthetized dog was exposed and a current of forty milliamperes made to traverse a portion of the ventricle. A piece of the ventricle in the direct line of the current was excised, and another some little distance from the direct influence of the current. When examined under the microscope the piece from the direct line of the current showed that the striæ had become markedly granular, while the piece outside of the direct line of the current preserved the muscle cells unaltered. It was stated that this was the first evidence of absolute molecular disintegration of the living cells by the interpolar action of the galvanic current where such process is confined to the cells.

Some deductions from the discussion by the editor of the Journal are noted as follows: '' Electricity will relieve the pain due to the pressure and sympathetic disturbances in the majority of fibroid tumors in from one to six applications treated by that agent. Hemorrhage due to fibroids can be relieved by the positive galvano-caustique applications of electricity in all cases in which a sufficiently concentrated dose can be applied to a greater portion of the endometrium.''

A large percentage of tumors of enormous size can be checked in growth and often reduced in size by an intelligent and persistent application of this agent, while tumors of medium and smaller size can be markedly reduced, and in a few instances be made totally to disappear. The majority of patients while under this form of treatment improve rapidly in general health from the characteristic tonic effect excited upon the whole system. It is said that the general tone of this discussion was practically free from the exuberant and unsafe positiveness of the enthusiastic hobbyist.

Dr. A. D. Rockwell speaks in an article published in the January number of the Journal of Electro-Therapeutics of the tonic effect of the electric current. Speaking of the faradic current he states that every electro-therapeutist is taught that even the induced current exerts some other and more subtle influence upon the nerves themselves, for by no purely mechanical means can we account for the numerous phenomena that follow its use in diseased conditions. It gives passive exercise to the muscles; it promotes and renders more natural the processes of excretion and secretion; it corrects circulatory disturbances—in a word, it imparts tone, strength to both nerve and muscle. The chemical or electrolytic influences associated with electrical action are practically confined to galvanism. He says it is undoubtedly true that some electrolytic action occurs in all ordinary applications of galvanism to the body, whatever form of electrode is used. A distinction has been made between electrolysis and galvano-chemical cauterization. The one is a disintegration and separation of the constituent elements of organized structure. The other produces its effects by means of the acid and alkalies that are liberated at either pole by electrolytic action.

Dr. M. Allen Starr, in a paper read before the New York Academy of Medicine March 21, 1889, concludes that "the constant galvanic current could produce chemical changes which aided nutrition or destroyed tissue, according to the strength employed. The constant galvanic current could transfer medicines in the body from without (cataphoresis). The interrupted galvanic or faradic current could excite various organs to functional activity."

Dr. Frederick Peterson, of New York, from a series of experiments on cataphoresis with, principally, cocaine and aconitine, concluded (see Dr. A. D. Rockwell's article in Medical Annual) that with the former or two combined a deep anæsthesia may be produced in conjunction with the anode. The anæsthesia may be made rapid with the use of strong currents or slowly produced with a current imperceptable to the patient.

Upon this subject we need not dwell further. Catalysis takes place in living tissue. Its precise nature is not understood. Cataphoresis, also a proof of catalysis, appears to be a clearly demonstrated fact.

The Resistance of Tissues to the Passage of the Electrical Current.

In the application of electricity in medicine the electrode applied to the body is usually a sponge saturated with fluid and connected with the metal portion of the electrode. The greatest resistance is at the point of contact with the body, and the character of the fluid used has much to do with overcoming the resistance. If the resistance should be considerable a cautery effect would be produced if the current be kept up a sufficient time, and series of burns might follow with sufficient electro-motive force. From a paper in "The Electrical World" of May 26, 1890, the average of a series of resistances on the human body, made by Dr. W. T. Stone, are as follows: Foot to foot resistance of three adults gave 935 ohms. The average resistance from foot to hand of same parties gave 1,126 ohms. These, from the context of report, are considered continuous current resistances.

In same paper experiments conducted by Mr. Wm. Lant Carpenter are interesting as showing the decreased resistance obtained by a steady continuance of application. Resistance taken from foot to foot with dry skin was 1,030 ohms; with salt and water to saturate electrode, in 1 minute, 4,300 ohms; in 10 minutes, 1,900 ohms; 20 minutes, 1,540 ohms; 30 minutes, 1,400 ohms; 40 minutes, 1,250 ohms; 50 minutes, 1,200 ohms; 60 minutes, 1,190 ohms to 1,200 ohms.

In the same paper, entitled "Alternating *vs.* Continuous Currents in Relation to the Human Body," by H. Newmann Lawrence and Arthur Harries, the authors present three series of measurements on ten adults of ages from 21 to 40 years. The electrodes, each of 50 square centimeters area, were used, the extended palms being placed on or grasping the electrodes. The conditions which might occur in accidental grasping of electrodes of conductors from dynamo currents were simulated as near as possible. The averages of the three tables given were as follows:

TABLE NO. 1.

Resistance to continuous currents:

Dry hands.	Moistened with distilled water.	Moistened with salt water.
38,140 ohms.	15,250 ohms.	9,557 ohms.

TABLE NO. 2.

Resistance to alternating currents:

Dry hands.	Moistened with distilled water.	Moistened with salt water.
4,155 ohms.	1,722 ohms.	1,365 ohms.

TABLE NO. 3.

Resistance to continuous currents:

Dry hands.	Moist hands.
14,475 ohms.	9,750 ohms.

Resistance to alternating currents:

Dry hands.	Moist hands.
1,740 ohms.	1,437 ohms.

One feature is prominent in these results, viz., that the resistance obtained by the alternating current is very much less than that obtained with the continuous current.

Mr. Thomas A. Edison instituted a series of experiments to ascertain the resistance of the human body to the passage of the electric force; 259 male persons, of various ages, weighing from 68 to 192 pounds, measured between the hands immersed to the wrists in caustic potash, aqueous solution, of density 1.1, gave the following results: Mean of all resistance, 986 ohms.

The deductions drawn from this extended series of measurements were as follows: The resistance of a man's body, taken between the hands, varies with *the solution* employed for immersion and the area of skin immersed, together with the superficial condition of the epidermis; that with a fixed solution, immersion, and area the resistance does not vary to any appreciable extent with the battery power used in the measurement, when the effects of polarization are eliminated; that with the K H O solution used, as given above, the resistance at about 30 seconds immersion is about 1,000 ohms. Why the resistance with the alternating current was not used is not stated. These experiments were brought about through the controversy on electro-execution, and from the great difference in resistance demonstrated by the English gentleman referred to previously between the continuous and alternating current it would have been of great interest to have compared the results in so extended a series of measurements. Few who use electricity in medical practice will question the following statements of Dr. A. D. Rockwell, taken from "Kemmler inquiry," that the resistance falls more rapidly when a low potential is used, and that the fall of the resistance, taking men ordinarily, would range between 1,200 and 400 ohms, and that this fall of resistance would be almost instantaneous; that after the current has once overcome the resistance, the resistance is gradually lessened until about the minimum resistance is reached, when it remains nearly stationary under a constant potentiality.

The Influence of Electricity on Sensation.

Any physician practically using galvanism in his medical work will find that 20 milliamperes will be generally painful to his patients. In fact, frequently a less powerful current will be objected to. It depends to a great extent upon the portion of the body operated upon. If a mucous membrane, even 5 to 10 milliamperes may prove very painful. As to the comparison between the painful effects of the continuous and alternating currents, a paper read before the Institution of Electrical Engineers, London, England, March 27, 1890, throws some light. (See "Electrical World," May 26, 1890.) The writers adopt 10 milliamperes as the maximum continuous current which may be passed through the body without producing unpleasant sensations. With alternating currents they found a great difference, inasmuch as before a single milliampere was registered their patients complained that the current was too strong and practically unbearable. In the course of their investigations they found but few persons who could bear with comfort one milliampere of alternating current. From their experiments upon ten persons they found that one and seven-tenths milliamperes (1.7) of alternating current was, on the average, all that could be borne without discomfort. Beyond that point violent muscular contraction, rendering relaxation of grasp first difficult and then impossible, produces distinct pain, which agitates the whole body. They state as their conclusions that the human body can bear with ease at least five times as much of continuous current strength as of alternating current strength. Dosage of electricity is somewhat similar in results to that of many dangerous drugs. If the dose is insufficient to kill, the result, owing to the influence on the system, may range from a slight to a most serious effect. Where the powerful doses of electricity have been received and the party survived, it is to be expected that recovery will be associated with painful sensations, which may vary greatly in degree. In railroad disasters, for instance, we have surgical shock, from that degree which produces momentary paralysis to that which produces apparent instantaneous death. This applies to many cases of recovery from powerful shocks of electricity. Testimony is quite uniform to the effect that no sensation was experienced by the patient at the time of stroke. This can only be accounted for by the assumption that the electric influence is so rapid in action that sensation cannot be transmitted to the brain centers before they are paralysed by the electric force.

I attended a meeting of the National Electric Light Association, at Niagara Falls, New York, August, 1889, to listen to an address opposing electro-execution. I went as a spectator, but was called upon to address the meeting. Among other statements regarding death by electricity, I said from my own demonstrations I could not help but come to the conclusion that no other agent we had at our command would execute criminals with such rapidity and without question of pain. I was then cross-questioned right and left, as I found I had struck a chord entirely out of unison with the views of the great majority of the members of the convention. This question: "I want to ask Dr. Fell if these several hundred deaths (referring to accidental deaths from electricity) were painless, and if on these points he rests his claim?" I answered by stating that Wheatstone had demonstrated that the electric current or force was manifested or passed at the rate of 280,000 miles per second of time. Further, that the nerve current or force was manifested or passed at the rate of about 111 feet per second, or some 2,500 or more times less rapidly; that when an electric current passed through the body it was hardly possible that the nerve current could overtake it even (allowing for difference in probable speed in conductivity of agent) or respond so as to produce a sensory impression on the brain centers, and that therefore death by a powerful electric shock, properly applied, would be painless. The following answer was made by Professor Anthony: "I have only a word or two to say. Dr. Fell made the remark that it was well known that Wheatstone demonstrated that the velocity of electricity was 280,000 miles per second. All of us know that such experiments were unreliable, and that it is now perfectly well understood that it was not the velocity of electricity at all, but simply the time required for those wires to be charged, and for a spark to leap across the gap between the two wires. The velocity with which electricity passes over any given conductor is not known, and we know that the velocity depends upon a thousand things. In the first place, it depends upon the conductivity of the conductor. In the second place, upon the amount of pressure or potential, and upon many other things. The fact is that upon the nerves of the human body the velocity of the electrical current is known to be immensely less than it is in a metallic wire. In a wet string the velocity of the current, in passing over even a space of a few feet, is readily measurable, requiring quite a large fraction of a second to pass over a distance of a few feet. I merely speak of this matter that it may go upon record, if these discussions are to be reported, that this question of the velocity of electricity cannot settle the point as to whether the death by electricity would be painless or not." I think it is generally understood that the term current, as applied, refers to electric force, and whether it takes a certain time to charge a conductor or to have the electric force or current manifested under the conditions mentioned is the same thing.

The testimony of many who have received powerful electric strokes and recovered is almost unanimous regarding the entire absence of sensation. In all cases where the cerebrum has been influenced this question may be decided in favor of entire loss of sensation. Had the sensory apparatus of the body time to act under these shocks, the recipient would undoubtedly be cognizant of it after recovery. I will refer to an experiment with the Kemmler chair later, having a bearing on this question of sensation.

The Production of Death and the Utilization of Electricity in the Execution of the Death Penalty.

The controversy occasioned by the passage of the electro-execution law by the State of New York has a direct bearing upon our subject. I propose to give tersely its history and my connection with it. The following report of experiments made by myself is copied from that of the commission appointed by Governor Hill, of New York State, to inquire into the most humane method of executing criminals. This commission consisted of Elbridge T. Gerry, Dr. A. P. Southwick, and Matthew Hale:

"In the month of July, 1887, there was conducted a series of experiments calculated to throw considerable light upon the powerful and injurious effect of electricity upon animal life. The authorities of the city of Buffalo, N. Y., had determined to rid the city of the numerous curs roaming the streets. To reduce their sufferings to a minimum, the agent of the Society for the Prevention of Cruelty to Animals recommended that electricity be applied as the death-dealing agent. The experiments were conducted at the improvised dog pond prepared at old police headquarters.

The canines were quartered in one room; adjoining this was an entry which communicated with a third room, in which the electrical apparatus was located. This consisted of a common pine box lined with zinc, and connected with one pole of the electric-light current for that portion of the city. When in use the box was partially filled with water. Connected with the electric-light wire, representing the other pole, was an ordinary dog muzzle, supplied with an iron or copper bit, which was inserted into the mouth of the canine. The animal being placed in the box, the switch making the circuit was turned, causing the apparent instantaneous death of the animal. Only in exceptional cases were any movements noted after the current was made. The result obtained by experiments conducted in this manner leaves the subject just where public opinion would place it, viz., "that electricity will kill quickly." However, to ascertain how quickly and thoroughly, requires further demonstration.

The heart may rightfully be considered the center of function, and in the execution of criminals by the legalized hanging process, is always examined to ascertain when death ensues. In favorable cases it is known that the heart may beat from six to ten minutes, and in some cases it has been known to beat from fifteen to thirty minutes after the drop. For the purpose of ascertaining the effect of the electric-light current on the action of the heart, the operation of opening the thorax of an animal under forced respiration was made. With the operation satisfactorily performed, the heart and lungs may be observed in action, viz., the heart beating and the lungs contracting and expanding as in life.

While the operation is not new to physiologists, still the effects upon the movements of the exposed heart by the passage of an electrical current which might be applied in the execution of criminals I do not believe has been frequently noted or the operation often, if ever before, performed. That the ordinary electric-light current used in these experiments is sufficient to cause instantaneous death of a human being is inferred from the many accidental deaths produced by such means. To witness the effect produced upon the heart in action is a demonstration which cannot be questioned, and offers a positive answer to what may have been inferential. To those favoring electricity as the proper agent in the execution of criminals, a demonstration of this character serves to make them more positive and less liable to be influenced by those whose investigations into the subject have been only superficial. Those opposed to it from the standpoint of uncertainty of action, it leaves without a foundation upon which to base their opinion. Prior to these experiments I held the view that electricity might prove the best agent for executing criminals; after they were made, I enthusiastically supported it as the only agent which this age had any right to use for this purpose.

But to refer to the experiments: A fair-sized dog was placed under the influence of chloroform, an incision was made in the trachea, in which a tube connecting with foot bellows and supplied with suitable valve for respiratory purposes was attached. Respirations were then kept up by these artificial means. The chest walls (thorax) were then removed, so that the heart and lungs were exposed to view; the dog was then placed in the zinc-lined box, the muzzle put on, the forced respirations kept up until just before the current was made. The heart was beating as in life, but the instant the circuit was made it ceased its action and became a mere mass of quivering flesh; not the least resemblance to a rhythmical movement was observed after the current was made. Many citizens present at the operation can testify to the above; in fact, the demonstration as to suddenness of stoppage of the heart exceeded all anticipation. The interference with all function was electrically instantaneous; death ensued from electric shock; the ordinary conditions of dying were absent; nothing could be more sudden.

This first experiment, although eminently satisfactory in its results, was made under conditions the most unsatisfactory. The rooms were full of men hurrying to and fro; the dogs were being led to their fate, and no suitable place was provided to operate. This and observations connected with another series of investigations led to a second demonstration, which was made under more favorable conditions, at the electric-light works on Wilkeson street, Buffalo. This operation was conducted with greater care. Instead of the muzzle, with the bit attached to make one pole of the circuit, a piece of wire line was placed in the mouth of the dog and wound around the nose. The zinc box was used as before; chloroform narcosis was produced and the thoracic walls removed. The heart was beating rhythmically. On making the circuit it instantly ceased to beat. The current was quickly turned off and forced respiration kept up with the view of bringing the heart again into action. This was entirely unsuccessful. The result demonstrates that if the current used is sufficiently powerful attempts at resuscitation in the case of a criminal executed by electricity would certainly fail. In this second experiment it was also noticed that an attempt to respire was made by the animal after the current was turned on. This undoubtedly indicated that the respiratory center in the brain (medulla) had not completely lost its susceptibility to impressions, and that, through the want of oxygen in the blood and center noted, the effort to breathe was formulated. This has an important bearing upon the apparatus to be used in executions, inasmuch as it indicates that the poles should be arranged to pass the current through the centers of function in the brain. Upon physiological grounds, also, this is indicated. Even without this refinement of precision in the apparatus, as has been shown in this last experiment, where the current was not passed directly through the functional brain centers, the sudden stoppage of the heart would indicate that electricity offers the most rapid agent in producing death that we have at our command. The mere estimate of the difference in the speed of the electric compared with the nervous current would further show that our senses could not interpret or apprehend the passage, or that death produced by such means would be absolutely painless to the culprit. From observations which are generally accepted, it may be stated that nervous force travels at a rate of from one to three hundred feet per second, while the electric passes at a rate of no less than two hundred thousand feet per second, or about one thousand times as rapid. From these observations the following deductions may be drawn :

"First. That death produced by a sufficiently powerful electric current is the most rapid and humane produced by any agent at our command.

"Second. That resuscitation, after the passage of such a current through the body and functional centers of the brain, is impossible.

"Third. That the apparatus to be used should be arranged to permit the current to pass through the centers of function and intelligence in the brain."

When this report was prepared it was understood that it was to go before the Legislature of the State of New York. It was the only record of actual demonstration in the report of the commission bearing upon the subject in question ; that it was influential in the passage of the electro-execution bill goes without the saying. Since the Kemmler execution I am more than satisfied with the truth of my conclusions. The report has been criticised as not covering electrical measurements. I was limited in apparatus, and accomplished as much as was possible with the means at my command.

To the physiologist, accustomed to demonstrate the action of the heart and lungs in life, it is noticeable that the heart does not spontaneously cease its rhythmical movements. It dies slowly, the movement becoming more and more labored until it ceases. To all who witnessed the experiments just reported, the sudden stoppage of the heart appeared to indicate a special influence of the current on that viscus. Subsequent experiments by many experimenters have proven the correctness of these conclusions.

The next series of experiments took place under the direction of Harold P. Brown, Esq., at the Columbia College School of Mines, New York city. A large dog was given 300 volts pressure, and then 1,000 volts of continuous current, without injury ; but 300 volts of alternating current caused intant death. August 3, 1888, at the same institution, another series of experiments were made by Dr. Cyrus Edson and Dr. Charles F. Roberts, of the New York board of health. The results of these observations, made to determine the danger of the alternating current, were as follows : A dog weighing 61 pounds, in good condition; resistance from left front leg to right hind leg, 14,000 ohms; two hundred and seventy-two volts, with 288 alternations per second, killed the animal; heart ceased breathing in 90 seconds; dog immediately dissected by Doctors Roberts and Peterson; sections of sciatic and pneumonogastric nerves, muscular fibres of lungs and diaphragm examined microscopically; no change in the structure observed.

A second powerful dog, weighing 91 pounds, was used; resistance 8,000 ohms; alternating current, 340 volts, applied for five seconds; dog silent during continuance of the current. For eight seconds after opening the current the dog howled and gasped from mechanical action, but was unconscious from the instant the current was applied; death in two minutes and fifty seconds.

The third dog, weighing 53 pounds; resistance, 30,000 ohms; 220 volts applied for five seconds. The result was not fatal for four minutes afterwards; dog rigid and motionless during continuance of the shock. The same current continued for 30 seconds, the voltage rising to 234 volts; dog died without sound or struggle. In this series of experiments the physicians present expressed the opinion that the dogs had a higher vitality, and that the current that killed a dog would be fatal to a man under the same conditions. It was their opinion that all of these deaths were painless, as the nerves were probably destroyed in less time than that required to admit the nervous impression to the brain.

As the heaviest of several dogs killed weighed but 91 pounds, it was claimed that the experiments could not be regarded as a criterion for the effect of the current upon a human being, and a further series of experiments were conducted by Harold P. Brown at the Edison laboratory. A strong and vigorous horse, weighing 1,230 pounds, and two calves, weighing respectively 124 and 145 pounds, were killed by the alternating current at 700, 770, and 750 volts. In all of these instances death is said to have been instantaneous and painless. A report of a committee of the Medical Legal Society recommended that the alternating current be used for the execution of criminals. The details that are interesting in this report are as follows: Regarding the application of the death current to man, it is stated that the average resistance of the human body is 2,500 ohms. In the application of the current for executions, the recumbent or sitting position was suggested. It was recommended that one electrode might be placed in contact with the head, and one electrode upon the spine between the shoulders, or, as I had previously reported on the conclusion of my first experiments, the current should be made to pass through the functional centers of the brain

A dynamo, generating the electro-motive force of at least 3,000 volts, should be employed, and a current used with a potential between 1,000 and 1,500 volts, according to the resistance of the criminal. In one of their experiments, upon the suggestion that the current should be applied through wristlet electrodes, upon, I presume, the theory that death would be as instantaneous as when applied to the brain centers, and so as to include the heart, they applied the current to the four legs of a horse, but found that the method was not nearly as effective as when applied to the head and back. These experiments are recorded in the appendix to the testimony (Kemmler inquiry) before the court of appeals, State of New York.

The next series of experiments became of intense practical interest, as the dynamo used was that provided by the State of New York for the execution of criminals at Auburn prison, and with which it was thought the first electro-execution would be produced. As I took a prominent part in this and a subsequent experiment at the same place, I will naturally be able to give a more detailed account than those with which I was unacquainted. The commission appointed to examine the apparatus preparatory to final purchase by the State consisted of Carlos F. Macdonald, M. D.; A. D. Rockwell, M. D., and Louis H. Landy, Ph. D. In addition, Gen. Austin Lathrop, superintendent of State prisons; Harold P. Brown, who furnished the apparatus; Mr. Charles F. Durston, warden of Auburn prison, and myself were also present.

From the report of the commission I gather the following facts relating to the power of the dynamo: The commercial voltage, 1,680; the mean voltage, 1,512; the maximum voltage, 2,376; speed of dynamo voltage, 1,700; speed of exciter, 2,700. In the alternating dynamo the maximum of the electro-motive force obtained in the rapidly changing alternations, or the mean of them, may be taken as the electro-motive force of the dynamo; hence the varying voltages given as above by the commission. Regarding the difference between the alternating current and continuous current, we may note in explanation that the continuous represents a steady electro-motive force in one direction, as in the galvanic current, while the alternating current, simulating the faradic or induced current at each alternation, proceeds from zero to the maximum and recedes to zero, and repeats this in the opposite direction. The definition given by the committee of the commercial voltage of an alternating dynamo is as follows: "Say fifty volts is such an alternating voltage as will, upon an incandescent lamp or Cardew voltmeter, produce the same light and heat effects as fifty volts in the case of a continuous current."

The commission had made their tests; wires were carried to a shed adjoining the prison. An old but vigorous horse, weighing some 1,200 pounds, had been secured. One electrode was secured above his eyes with cords, and the other was also attached above the knee-joint of left hind leg. The electrodes were of copper plate, with cotton waste attached; the cotton was saturated with warm water, which was poured over them after being attached. One noticeable feature on this occasion was the anxiety of all present regarding the ontcome of the experiment. All the gentlemen present had witnessed the death of the animals by electricity previously, and believed in the efficacy of the agent. However, this was the first experiment with the dynamo which was supposed would soon be used in the execution of a human being. The experiment being made upon a large horse, a slight failure would not, of course, be so serious a matter, as the resistance of a horse with the current passing through the whole length of the animal's body would naturally be very much greater than that which would be presented in the body of any culprit; but it would have dampened the ardor of all interested in a successful result. Noticing a diffidence on the part of the gentlemen present in taking active part in the experiment, I took off my coat, donned my physiological laboratory apron, and went to work. I was requested to turn the switch, and all breathlessly watched with interest the result of the shock. The horse by this time was standing quietly in one corner of the shed, undisturbed as to what was going on about him. When it was ascertained that the dynamo was in full operation, I took my stand upon the box, and, with my hand on the switch, watched the horse as I made the connection. He immediately stiffened out and fell to the ground dead. The current was kept up twenty seconds. There was not, in fact, a move on the part of the animal. The moment the current was turned off, with several assistants I removed the electrodes attached to the animal, made a slit between the ribs, in which I thrust my hand, with the idea of detecting any heart action. There was none; death had been instantaneous. A good-sized calf was then taken and the electrodes applied to the head and spine, as I suggested. I turned on the current, and, as before, the animal was instantly dead. The current was turned off, having been acting ten seconds; electrodes were removed. I made tracheotomy and applied *forced respiration* with the same instrument with which I have saved six human lives. It was kept up for half an hour, and at no time was there any heart response. I desire to state that this animal was placed under the best condition known to medical science to live after the shock had been given. I made this experiment, not with any belief that the animal which had received such a powerful electrical shock could be revived, but merely to satisfy those who had been influenced by the ignorant statements made in some portions of the "Kemmler inquiry," to the effect that animals which had received a large dose of electricity might be revived if placed in the ground for a period of time until the electricity, said to have saturated them, had been drawn from them by the moist earth.

These experiments, as all the others I witnessed, presented no feature of uncertainty; no sound or cry was made by the animals, and, as I had formerly advocated the use of electricity as a death-dealing agent, I could not but feel satisfied with the results.

And now comes the question of the method of application of this agent to the execution of a human being. At the time these experiments were made the electro-execution plant at Auburn was in a decidedly chaotic state. Considerable labor had yet to be expended to place it in a satisfactory condition.

THE KEMMLER CHAIR.

No chair suitable or accepted by those in authority for the purpose had been prepared. One illy adapted to the purpose was lying in the vaults of the prison, known as the Harold P. Brown chair. As the execution of Kemmler had been ordered by the courts, it was natural that those in authority should feel the necessity of prompt action. Following the above experiments, I explained to Gen. Austin Lathrop, superintendent of State prisons, my views relating to the features of the chair to be used. At his request I subsequently made drawings and specifications, and was requested to have a chair made according to my views. I carried out the idea I formerly expressed, viz., that the current should be made to pass through the centers of function and intelligence in the brain, etc. One electrode was placed over the cerebrum; the other against the spine, in the dorsal or lumbar region.

The dissemination of the current with electrodes thus applied would include the heart and produce the greatest density in neck, including the region of the medulla oblongata. February 12, 1889, the chair was shipped to the warden of Auburn prison, Mr. Charles F. Durston.

At the request of General Lathrop I had kept the manufacture of the chair a secret, so that no notice had appeared regarding it. It resembles an ordinary heavy oak arm-chair, with perforated, wooden seat, the cross-pieces at lower portion of back removed to give room for spinal electrode. Two upright pieces at the back of chair permit a third upright piece to move between them. This carries a strong arm at upper end, which projects forward over the head of the culprit and carries the head electrode, which may be secured at any point by simply turning a binding screw, which secures the upright piece supporting the electrode ; by this means the chair can be used for a large or small individual. A foot-rest, which can be pulled out from under the seat of the chair, has a cross or body piece provided to prevent the feet or body from sliding forward when a current is applied and rigor of the muscles ensues. This foot-rest was not used at the execution, and, from the experience obtained, is not needed. Without this, however, the straps supplied secure the body from moving. Straps are provided to secure the arms and limbs from movement. One passes around chest, holding the upper portion of the body from moving forward. A combination chin and forehead mask prevents any movement of the head.

The spinal electrode is held in place by a strap which is attached to each arm of the chair ; another strap secures it to the criminal, while an abdominal strap pulls the body backward against it. The head electrode is provided with a spring which takes up any downward movement of the head if it should occur, and, with the spring of the rubber cups forming them, the combination effectually prevents any possibility of the culprit executing a sudden movement to escape contact with the electrodes. The electrodes are circular, four inches in diameter. Each one is surrounded by a rubber cup, with the edges of its sides flattened so as to fit closely the body or head. The electrodes are brass perforated plates covered with sponge, and secured to the large wire which passes through the rubber cups to be connected with the dynamo wires. When the electrodes are in place and properly arranged the edges of the cups press against the head and body of the culprit. Openings are provided so that the saturating fluid may be applied with a long rubber pipette, with large bulb provided for the purpose, at any moment previous or during the passage of the current. The spinal electrode can easily be attached to the criminal before he is seated in the chair, and connected with the dynamo wires later, if desired.

In a letter to General Lathrop describing the chair, I stated as my belief that it would accomplish the work for which it was constructed, *if sufficient electro-motive force was used*, and in such a manner as to take away from public executions the horrors which frequently attended them. Since the execution of Kemmler I am more than satisfied of the truth of this statement. In my laboratory I had passed a galvanic current through the electrodes when applied to individuals who had allowed me to experiment with them, and thus demonstrated that the method devised for attachment of electrodes would offer a minimum of resistance when suitable fluids were used. In these cases an instantaneous flash of light was noted on the make and break of the current. With five volts electro-motive force and a current of five milliamperes a flash would be produced with the most rapid making and breaking of the current, demonstrating (so far as the senses could appreciate) instantaneous passage of the current through the body and indicating in degree the rapidity of death by this method. By accidental contact 10 to 12 volts were passed through a young gentleman in the chair ; he was stunned, almost rendered unconscious, and quite thoroughly frightened. When it is considered that the current as applied passes through all the vital centers of the body, beginning at the cerebrum, the center of intelligence, and taking in the cerebellum, medulla oblongata, heart, and lungs by diffusion, and spinal chord, the effect of the powerful current of a dynamo upon a culprit may be appreciated.

I wish to state it as my belief that a current with 50 or 100 volts e. m. f., occasionally alternated, passed through the body in the course provided by the Kemmler chair is more than a human being could stand for any length of time. The remarkable statements of men living after vital portions of the body had received 800 to 1,000 alternating voltage I do not credit for a moment.

In sequential order the next experiment took place at Auburn, New York, during the week beginning April 28, 1889, when Kemmler had been sentenced to die according to the new law. The witnesses to this proposed execution, as is well known, through legal processes, did not see Kemmler die. There was a natural desire to see the power of the current on an animal, and Mr. C. F. Durston procured a good-sized calf for this purpose. I took entire charge of this experiment, and know from experience that the attachment of electrodes weighted down with a long, heavy wire to the head and spine of a calf is no easy task. The calf was overcome and quietly lying on the floor when Mr. Durston (at my request) turned on the current of about 1,200 volts e. m. f., instant rigidity of the animal resulted. In ten seconds I gave the signal to turn off the current; the rigidity passed away; the animal had been instantly killed. Previous to the application of the current I secured some blood from the animal; about an hour after I also took some. Microscopical examination revealed that the corpuscles taken subsequent to death were markedly crenated, which, on comparison with blood taken from a calf killed by a butcher, I believe, however, to be a postmortem change.

As by experiments on the lower animals we can only arrive at conclusions regarding the death-dealing influence of electricity, I wish to refer to the work of Dr. Edward Tatum, of Yonkers, New York, made at the University of Pennsylvania.

Dogs were the only animals used. Experiments were arranged for the purpose of ascertaining how definite a relation might exist between density of current and fatal result. The procedure was as follows in the case of each dog: With the closure of a current that was not expected to be fatal, and then to follow that with closures of successively increasing strength until the heart was arrested. The duration of each closure was one second; and a subsequent dose was not given until the heart and respiration were apparently restored. The electrodes were flexible frames of fine copper wire, covered with a thick coating of absorbent cotton, and were wet with weak salt solution. A round one, about two inches in diameter, was bound on the forehead, and a rectangular one, one and one-half by six inches, was bound upon the inner aspect of the right thigh, as close to the body as possible.

In a letter from Dr. Tatum, of March 15, 1890, in response to an inquiry, he says:

"It is understood, of course, that my results are in strictness only applicable to dogs.

"These results are, in brief, that with currents of between one and three amperes the heart is distinctly stopped before the current has produced any other discoverable lesion; and this, even after division in the neck of both pneumogastrics, or the profound poisoning of their terminations in the heart substance by curare or atropine, or even after the chest wall has been opened. The stronger the current the more distinctly and independently is the separate action of the heart recognizable, and the more unavoidable is the conclusion that the result flows from a direct action on the heart substance of that portion of the current which actually traverses it.

"Currents of less strength than about one ampere may require from one to many seconds before the heart is arrested. As this interval is lengthened the interference with pulmonary respiration assumes greater importance, until a point is reached when it may be said that death results from simple suffocation. These results were obtained with the sort of currents that are used commercially, namely, continuous currents, as well as very rapidly alternating ones.

"The strongest current I have applied to a dog was about three amperes, requiring a pressure with carefully applied electrodes of between 900 and 1,000 volts, and consuming about four actual horse-power. Such a current has killed when continued for only one-eighth part of a second. The maximum volume of mixed gases that this current could liberate in that time, under the most favorable circumstances, in a sulphuric acid voltmeter, would be less than one-fifth of a cubic centimeter (at the temperature of the body). This power is equivalent to the liberation of heat of 0.71 kilogram degrees, centigrade, in one second, and in one-eighth second would be able to raise the temperature of a 15-kilo dog about the one hundred and fiftieth part of one degree centigrade."

In a letter dated May 27, 1890, Dr. Tatum refers to his article in the *Electrical World*, and refers to certain facts which they seem to prove, as follows—some of them, it will be noted, have an important bearing on the Kemmler case:

"1st. In only three of the twenty-four dogs killed did the heart fail to be arrested distinctly before respiration. In these three no priority could be assigned to the failure of either function. But in the twenty-one other dogs effective respiration survived the final heart arrest. It often began with normal or slightly exaggerated force and good rhythm, then died out more or less gradually, but with no final convulsions. Fair inspirations were recorded in several cases as long as four or five minutes after a dose which had lasted only one second, but after which the heart had not executed a single beat that could be detected.

"2nd. Contrary to what I believe has been the general impression, as well as my own, the two cases in which I used currents with 120 reversals per second seem to indicate that if fatal results are at all dependent upon the rapidity of alternation, 120 reversals per second are slightly more mischievous than 300.

"3rd. Alternating currents, as has been long supposed (though on what seemed to be insufficient evidence), can, for a given time and mode of application, bring about a fatal result with only a fraction of the electrical output required by continuous currents; but even here a certain respectable density of current is required, for the least neck density of alternating current that proved fatal was somewhat greater than one-fifth of the least fatal density with continuous currents.

"4th. The noteworthy fact was developed that under ether, this difference in fatal power between continuous and alternating currents either entirely disappears (4 dogs) or is at least conspicuously lessened (2 dogs)."

The Doctor then attempts to account for the difference in fatal power under ether in these words: "It seems to be both a necessary and an adequate explanation to refer it entirely to the well-known superiority of interrupted or alternating currents as nerve excitants. This property normally enables alternating currents to add to whatever direct physical action they may have in crippling the heart muscle, all the physiological energy of the cardio-inhibitory nervous mechanism; but, of course, it loses its importance just in proportion as the nervous control of the heart is weakened by ether.

"The fact to be emphasized is that this superiority of alternating currents is not owing to any kind of lesion or paralysis or exhaustion of any part of the nervous mechanism, but rather to the calling into action of a truly physiological function, and depends for its manifestations upon the integrity of this mechanism."

He says: "In spite of the strong general opinion against my position, I had been able, until this last series of experiments was undertaken, to believe that there was no important difference between the fatal powers of continuous and alternating currents (when either effective voltage or effective current was considered); that the modes of action were identical, and that therefore I had only one sort of death to account for. This happened because when I had originally determined that continuous and alternating currents were of equal power under ether, and that continuous currents were not influenced by ether, I had falsely inferred that the effects of alternating currents were equally independent."

The experiments discussed in the Doctor's article in the *New York Medical Journal* of February 22, 1890, "seemed to show clearly enough that, under the conditions there noted, death was not caused by any sort of tissue lesion outside the heart; but these ether experiments proved that, with normal dogs and alternating currents, no lesion whatever can be taken into account; for under ether dogs can tolerate without injury currents three or four times as great as have ever been survived without ether; and it would be absurd to suppose that ether could protect any tissue against physical lesion from alternating currents, so as to reduce their power exactly to that of continuous currents, and yet have no effect on the action of continuous currents.

In one of these experiments under ether 1.05 ampere alternating current was passed through a dog from forehead to thigh (after four other applications gradually approaching this in strength). This is just three times as strong as the strongest current that I have yet used in killing a dog without ether. It is four and three-quarters times as strong as any dog has survived without ether; yet one hour after this surprisingly large dose the animal, being fairly recovered from the ether, the thing abnormal in his condition was a mild, general, rhythmical tremor. At this time, also, after a little coaxing, he walked across the room and drank some milk. The next morning the dog presented no sign of having been misused, but in disposition and demeanor was entirely natural. He so remained until the fifth day after the large dose, when he succumbed to .245 amperes, after these other doses, gradually approaching this strength, or less than one-quarter of the current survived under ether."

We will presently note how the microscope bears out this interesting demonstration, to the effct that lesion, in the ordinary sense, is not necessary to the production of death by electricity; and, further:

Dr. Tatum's paper, February 22, 1890, *New York Medical Journal*, regarding some of his experiments, says: "Whereas a current of one ampere, passed in either direction for one full second, between the head and thigh of a dog weighing not more than sixty pounds, and representing in the neck a current density of as much as a ten thousandth of one ampere to the square millimeter section, invariably causes immediate and permanent arrest of both heart and respiration, yet a current of two and a half amperes has been passed for several seconds from one hind leg to the other of a much smaller dog, and representing a current density ten times as great as that just mentioned, and the dog has immediately afterward risen to his feet and walked away."

"From this and the negative results which the microscope gives in examination of the heart of animals killed by electricity, we may say with the doctor, that somatic death may be caused without serious lesion of either substance or functions of muscles or nerves, but also that when such lesions do occur they are in no sense the cause, direct or indirect, of death, and can only have resulted from a great excess of current above the fatal strength. A dog was killed by passing a smooth, continuous current of 0.4 ampere between the head and thigh, the positive pole or electrode being applied to the head. The current was then maintained of the same strength and direction for one hour and forty minutes longer, interrupted only by nine momentary breaks, made for the purpose of testing muscular contractility. By the end of this time the whole body was in a fairly firm rigor. This had been first noticed at the end of forty-five minutes, beginning in the neck, extending then to the muscles of the back, and appearing in the thigh, to which one electrode had been applied rather sooner than in the other extremities. Yet after this prolonged application of a fatal dose there was no sign of tissue disintegration or of any liberation of gases in the tissues or in the blood-vessels; nor any lesion whatsoever except a light ecchymosis between the skull and scalp immediately under the electrode. If there is any change at all wrought in the blood, then it is not in the nature of ordinary electrolysis; nor is it of a sufficiently gross character to be readily detected with the microscope; nor does it evidently alter the physiological character of the blood. Until, at least, the existence of some change is proved, it is useless to speculate on its possible nature or consequences.'' This does not correspond with the result of the examination of blood taken from William Kemmler. (See Plate. The current after the second application must have produced its effect on the blood through its thermeric influence.

The doctor further says: " Respiration may be suspended or inhibited without the immediate arrest of the heart; and, on the other hand, the heart may be instantly and definitively arrested, while the respiratory mechanism yields only gradually. A current of one ampere passed between the head and thigh for one full second, in either direction, or an alternating current of the same virtual strength for one second, has always stopped the heart beat and respiration at once. And, further, the most fatal mode of application has been when one electrode was placed immediately over the heart region, for in three experiments where this plan was tried the heart was stopped by a strength of current and a duration of closure decidedly less than ever sufficed when the current was passed from the head to the thigh.

Dr. A. D. Rockwell has kindly expressed for this paper his opinion on the subject of death from the electric shock: "In regard to the cause of death by the electric shock, I do not feel competent to say more than that it seems to instantly paralyze the heart through its action on the nerve centers. I have witnessed a number of post-mortems after accidental death by electricity, and in every case there was an absence of even a suggestion of tissue lesion, the heart only excepted, and even in this organ the minute effusions following capillary rupture were due to its sudden and powerful contraction, and not the direct action of the current itself."

Now, what does the microscope say?

Interesting in this connection is the report of Professor S. H. Gage, of Cornell University, upon the result of examination of the heart of a calf killed by the Kemmler chair electrodes and Auburn plant:

"In examining the calf's heart muscle which you sent me, I made sections, stained and mounted in balsam. Part of the tissue was isolated with caustic potash, and part simply dissected with needles and mounted in glycerine. In order to have a criterion to guide me, the heart of a calf butchered in the ordinary way for food was obtained, and examinations made while it was fresh, and then after treatment with alcohol, as described, for the heart received from you.

"In the examinations, preparations were taken from the same part of the heart in the two specimens and treated precisely alike.

"RESULTS.—After comparing parallel preparations, made as described above, I am compelled to say that no constant differences could be found. All the examinations were made finally with a Zeiss Apochromatic, $\frac{1}{12}$, ocular X 12.

"There was an indication in the balsam preparations of a difference. In the heart killed by electricity the longitudinal striation of the muscle cells was very clear, the fibrillæ seeming to be separated by a comparatively wide, clear line, and in the fibrillæ the dark band was very marked, giving the appearance of a row of light and dark cubes. From the width of the interfibrillar light line the transverse striation was not so marked as the longitudinal. Later the distinction broke down, as similar if not quite so marked appearances were found in the heart muscle of the butchered calf.

"The nuclei of the muscle cells were scrutinized with the greatest care, but no difference could be discovered. I am sorry not to be able to give a more satisfactory report, but the subtle fluid seemed to kill without leaving gross enough marks for me to detect."

The result of examination by William C. Krauss, M. D., Professor of Pathology, Niagara University, Buffalo, New York, of brain of same calf killed at Auburn: "The left hemisphere and cerebellum, immersed in alcohol, were presented me for microscopical examination.

"MICROSCOPIC EXAMINATION.—The specimens are in good state of preservation; pia is not adherent; pial vessels are somewhat injected; convolutions present no abnormalities; brain substance is firm and resistant, not brittle, and shows no petechial extravasations on section. There is a slight discoloration of the pia and underlying brain substance over the frontal and occipital lobes. This, no doubt, was produced by the electric current, and the discoloration, in all probability, is the result of thermic action.

"The brain was further hardened in alcohol for three weeks, and small sections taken from the frontal, parietal, temporal, and occipital lobes were imbedded in celloidin preparatory for cutting. No difficulty was experienced in cutting very thin sections. The staining methods used were ammonia, carmine, hæmatoxylon, and Neissl's magenta red.

"The sections took an indistinct diffused stain; the ganglion cells, etc., lacked that sharpness and clearness of outline which characterizes normal brain tissues. Whether this was the result of chemical change or to some fault in hardening, I am unable to say.

"As to the physical condition of the ganglion cells, there appeared to be no material change; nucleus, cell body, and poles were in normal condition. The same was true of the vessels and neuroglia cells. No evidence of hemorrhage into the brain tissue could be discerned. The periganglionic spaces were found free and unobstructed. The result of the microscopic examination is, therefore, negative, as far as the physical condition of the separate brain elements are concerned. Whether the diffused appearance of the sections can be attributed to some chemical change in the protoplasm, I leave unanswered."

A micro-spectroscopic examination of blood by Prof. John A. Miller, Medical Department Niagara University, Buffalo, N. Y., resulted as follows:

Samples of arterial and venous blood of William Kemmler, obtained Saturday, August 9, 1890, from Dr. C. M. Daniels, of Buffalo, were submitted to spectroscopic examination. Both specimens showed the absorption bands of oxhæmoglobin situated between D and E of the spectrum. The position of the lines was identical with those of my own blood. The lines of the arterial blood were identical with those of the venous. The addition of ammonium sulphide caused the band of hæmoglobin to appear very distinctly. A very interesting feature in regard to these samples is the fact that the reaction of the blood instead of being alkaline is acid.

That these are interesting and valuable results, no one will question.

The conclusion that special lesion of tissue is necessary to the production of death by electricity appears to be answered in the negative so far as the microscope is concerned.

The First Electro-Execution.

The execution of William Kemmler having been set for the week beginning August 4, 1890, on the 5th instant the "committee of reputable citizens" provided by law met at the call of Warden Charles F. Durston at Auburn prison. Your humble servant was constituted an official "reputable citizen" for the first time in his life. Lately, through the press, you may have heard something about their doings at Auburn. Without entering into the preliminary details of the execution, I will proceed at once to give a short account of this first official taking of human life by electricity. Since the former proposed execution, changes had been made in the location of the apparatus to be used for this purpose. When the calf was killed at Auburn, the entire plant of the execution was in one room, with the exception of the dynamo, which was some two or three hundred yards distant, in a separate portion of the prison. Communication with the engineer was by an electric bell. The chair had been removed to another room, so that to witnesses of the execution there was nothing whatever to indicate when the current was working favorably. This I believe undesirable. The lamp-board, the switches, ammeter, and voltmeter were in an adjacent room. The intent of this arrangement was that it might be concealed from the world whom the individual might be who turned on the fatal current. The chair was arranged in the center of one end of the room and securely fastened to the floor and perfectly insulated from it. One wire passed to the spinal electrode, and the other was carried up to the ceiling and brought down to the cerebral electrode. The attachment of the spinal electrode had been modified somewhat by Mr. Durston; a spring having a play of some two or three inches was arranged so that it would hold the electrode in connection with the body of the culprit, so that it was impossible to draw away from it. As the chair was arranged, it was demonstrated that the electrodes could be closely applied to the body; that upon the back, however, not having been as thoroughly supplied with the saturating fluid as that upon the head.

The details of the culprit's actions in the trying ordeal to which he was subjected I need not repeat, but merely say that William Kemmler went to his death in a manner which won the admiration and respect of all who beheld him, and demonstrated the untruthfulness of many reports which had been circulated about him. However, had he been a powerful, strong individual and objected, there would have been no uncertainty about the carrying out of the penalty. Once strapped in the chair, the most powerful man could not have interfered with the purpose of the law.

In the audience, composed of some twenty-four or twenty-five gentlemen, there were physicians accustomed to sights associated with death. There were others who were incapable of witnessing even the culprit in his chair without fainting ; also some interested in giving to the world as sensational an account of the occurrence as was possible.

From reports circulated in the daily press, I note that Mr. Edison objects to the location of electrodes on the head and spine. I am inclined to believe that he has given credence to the many unreliable reports published about the execution. There was no question raised about the ready passage of the current. In fact, as will be noted presently, the electrodes as applied presented the strongest evidence that their location was well advised.

Mr. Edison is reported to base his argument in favor of passing the current through the hands upon the vascularity of these parts. He is reported as saying, "The arms, hands, and fingers are full of blood, and the current should have been forced through them."

It must be noted that we have in the lacunæ, canaliculæ, and haversian canals of the bone a net-work, filled in life with blood, and presenting a vascularity that will compare closely with that of the softer tissue, and thus presenting a very fair conducting medium. The question of paralysis of the psychical centers of the brain, which is obtained by the location of the electrodes, as in the Kemmler chair, is, in my opinion, of vastly more importance than to pass the current through the body with the positive uncertainty of the interference with consciousness, which might ensue if the current was passed through the arms. The difference, if any, in resistance is a factor of inconsiderable consequence in electro-execution, and it did not appear to be of importance in the Kemmler case.

In the case of Kemmler, the head electrode was filled with the potash solution by an attendant and myself, and, through the spring arrangement which secured it in place and the immovability of the culprit, there was at least one inch in depth of fluid covering the electrode and confined within its rubber cup. In other words, there was no leakage of fluid from this electrode. How interestingly does this answer the question as to the horrible writhings, twistings, and contortions of the body which were reported to the public, and by many reputable medical journals accepted as truthful. "The truth will out." How did I know the electrode did not leak? I removed it myself some twenty minutes after the current was turned off, and the fluid poured down over the head. In this electrode the hair at one edge was merely singed, where an arc had been formed. It was not burned to any extent, not anywhere down to the scalp. There was no question of imperfect contact raised here.

The electrode on the spine did not fit as tightly or hold the fluid so securely, but even then it did its work; but, after the third application of the current, dried up the saturated sponge, which produced the smoke as it burned away (only at one edge, however), thus allowing the brass plate to touch the skin at this point only. A burn, of course, resulted; but Kemmler was a corpse some time before the second application of the current. Will any one question, even if they do not desire to admit, that Kemmler died in the first twenty seconds; that he was not dead at this time, one hundred seconds after first application of current—dead without physical suffering, also? This is the truthful statement of the result of the first electro-execution by one who during its enactment was from two to six feet distant from the culprit.

The true history of the first electro-execution should read thus: Current applied; unconsciousness; death immediately resulted; current kept on about ten seconds—too short a time. Within *20 seconds of first application of current I could detect no pulse* at wrist. Shortly afterwards two or three slight movements of chest took place. After the first application of current there was not a movement, except as above, of a flexor or extensor muscle of either limb or arm; not a twitch of the muscles of face; no reflex action of eyes on removal of head-strap; not, in fact, a move, except what was produced by the make or break of the dynamo current. The man was alive one moment, and dead, so far as the ordinary evidences of death are concerned, the next. Reflex phenomena may be present after death, as is well known, and nothing more than this was present in Kemmler after the first application of the current. As to the import of chest movement, see Dr. Tatum's results on dogs, already quoted, viz: "In 21 out of 23 dogs killed by the application of electricity, effective respiration survived the final heart arrest. Fair inspirations were recorded in several cases as long as four or five minutes after the dose, which lasted only one second, but after which the heart had not executed a single beat that could be detected." My own demonstrations point to the same conclusions as in the second dog operated on—the heart ceased beating instantly, but attempts at respiration were made afterwards.

From these reasons I cannot agree with Dr. Shrady, one of the few witnesses who reported adversely to the method, as to possibility of resuscitation of Kemmler at this time. See also testimony, No. 3777, Kemmler inquiry, where a Mr. Smith respired some time after contact with brushes of a dynamo, which destroyed life. There is also plenty of evidence to show that respiration following heart arrest has been kept up in individuals subjected to powerful electric stroke for some time where resuscitation was impossible and the heart had ceased to beat. However, the current was re-applied within 70 seconds. As might be expected, when its influence reached the muscular coats of stomach, a contraction took place, causing a small amount of mucous to ooze from the mouth—not fly all over the room, as one report states. This is a good indication of death, also. One hundred seconds had now elapsed, *at farthest,* since the application of the first current. But Kemmler was dead at the first application of current, and with not one iota of feeling, as I stated, at the time.

It can be truthfully stated that the first electro-execution, inspired in the interests of humanity, and with the methods employed, has demonstrated the truthfulness of all that has been claimed by its advocates. If criminals *must be executed* there is no method so certain and ready of applicability if the apparatus is properly provided.

I will here leave to those who have followed me through this discussion, so incompletely presented, the privilege of drawing their own conclusions, having no question in my mind as to the result.*

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* I am indebted to Mr. Henry L. Tolman for the photo-micrographs of blood which accompan this article.—G. E. F.

Plate I. No. 1.

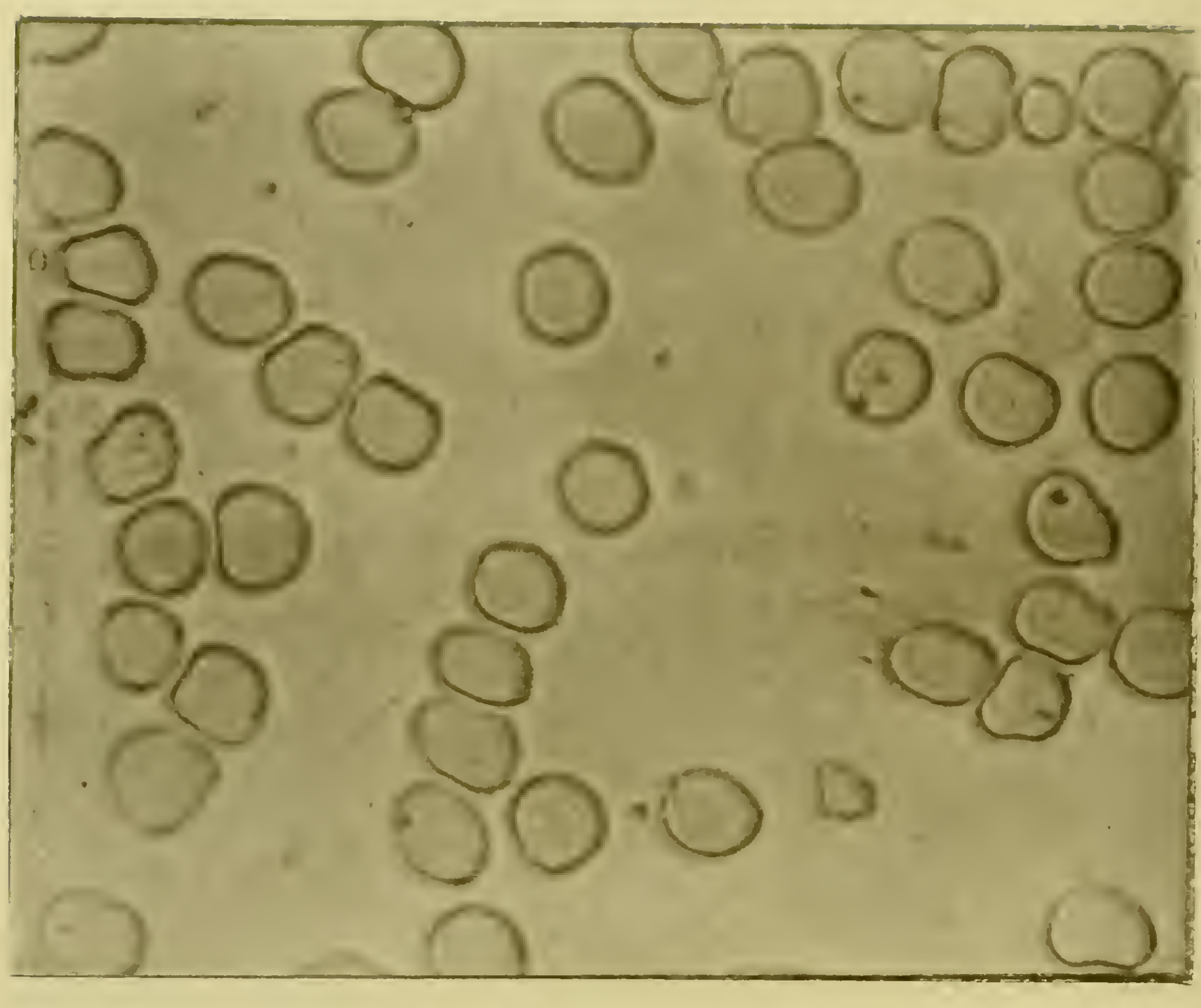


Plate I. No. 2.

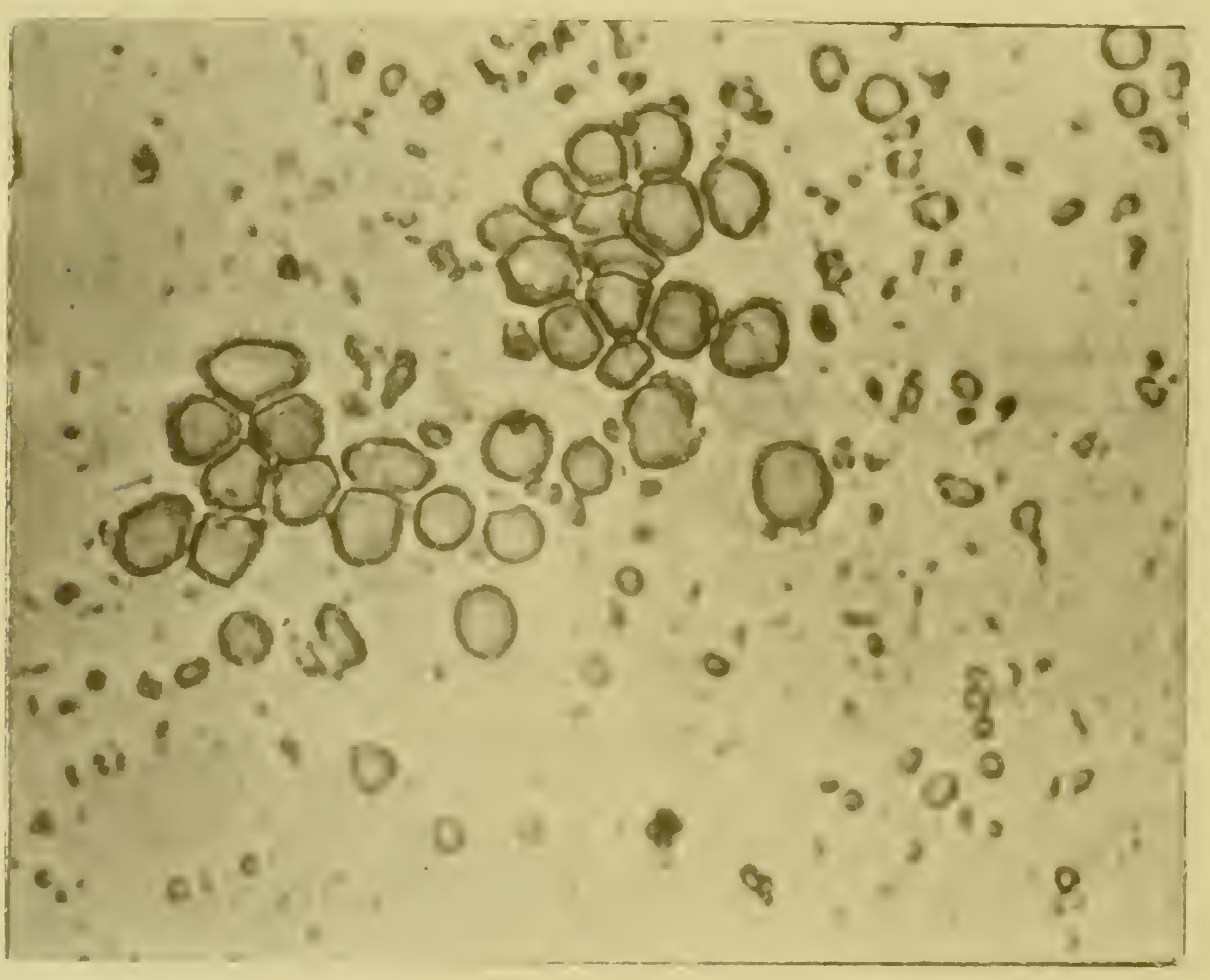


PHOTO-COLLOTYPE, HOFE M'F'G CO., PHILADELPHIA.

Description of Plates.

PLATE I, NO. 1.—Photomicrograph of blood taken from outer aspect left thigh half an hour after the execution. This portion of the body did not come under the direct influence of the current. The corpuscles present a normal appearance as to size and uniformity of outline.

PLATES I AND II, NOS. 2, 3, AND 4.—Photomicrographs of blood taken from left temple seven minutes after the execution and almost directly under the influence of the current. Corpuscles markedly reduced in size, in many cases to merely granular particles of protoplasm and with irregularity of outline. Nos. 2, 3, and 4 taken from different sections of same slide.

I am indebted to Mr. Tolman, of Chicago, for the photographs of the Kemmler blood.

G. E. F.

Plate II. No. 3.

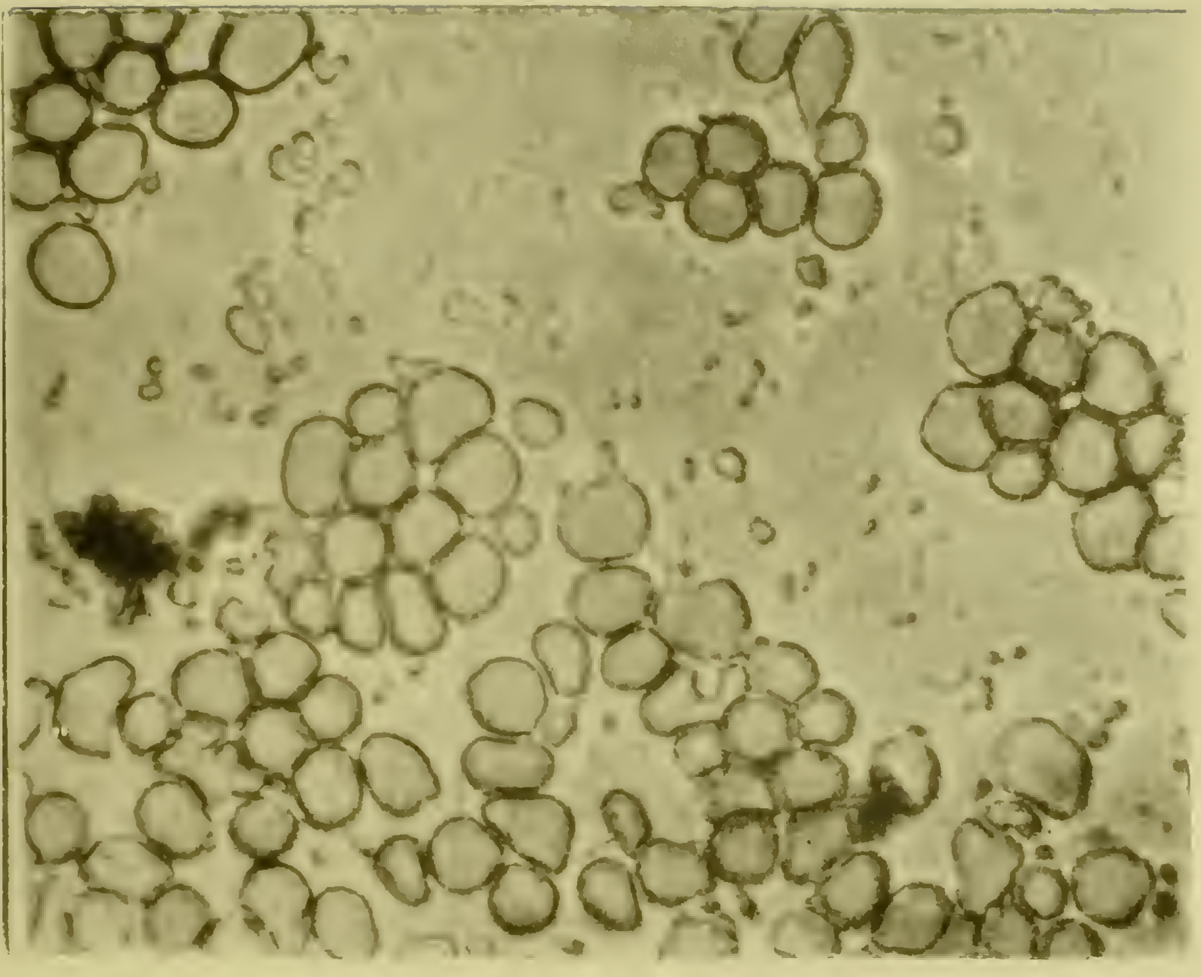


Plate II. No. 4.

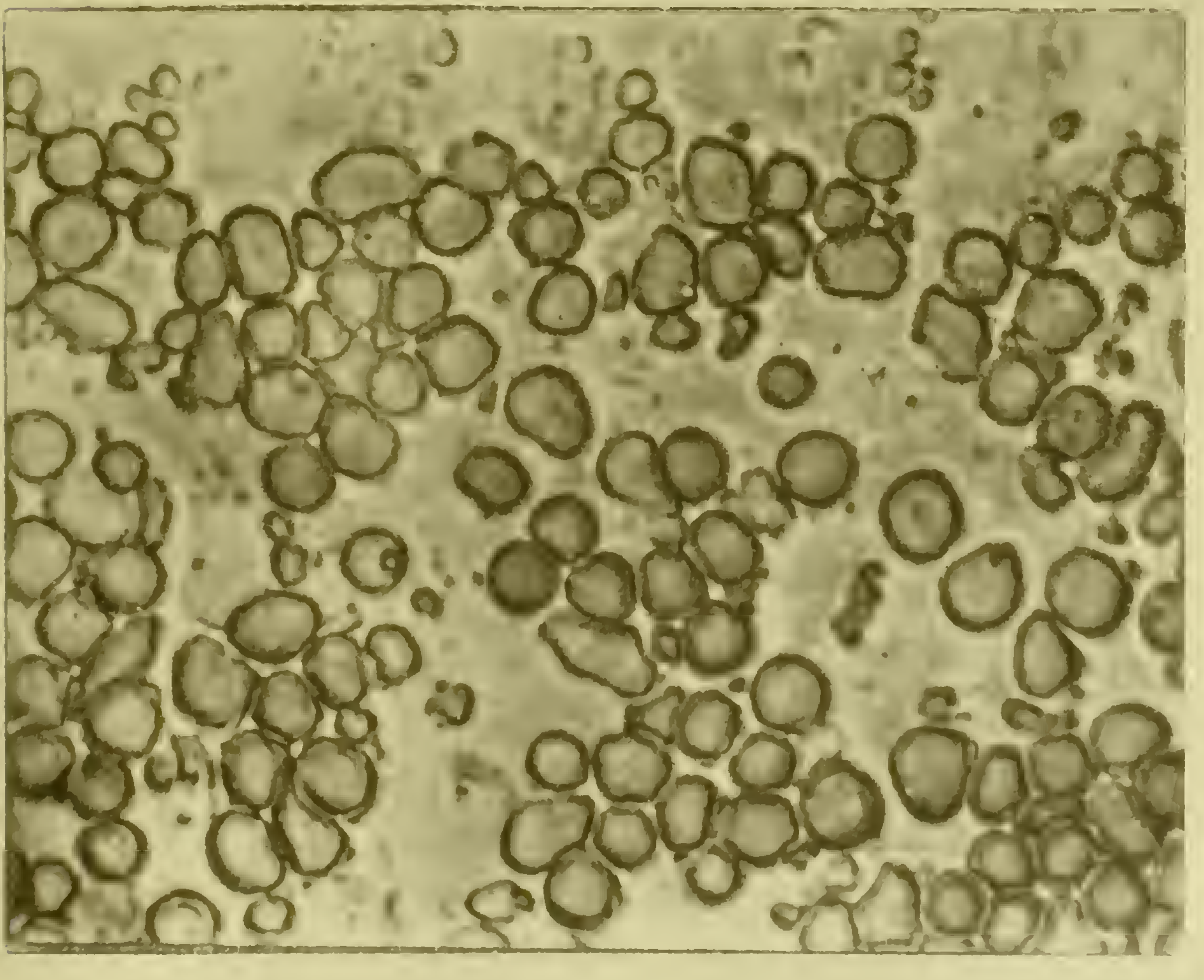


PHOTO-COLLOTYPE, HOPE M'F'G CO., PHILADELPHIA

MICROSCOPE OBJECTIVES.

T. J. Burrill, Ph. D., Champaign, Illinois.

I had the honor of presenting to this Society at its last meeting a paper embodying my experience and opinions concerning the microscope stand. I now wish to offer the results of personal experience in the use of various objectives for microscopical work, especially along the lines followed as a teacher and investigator of biological science.

The task thus set before me is more difficult than that of last year. Little niceties of difference count much more in an objective than in the construction of a stage, or rack and pinion adjustment; and, though one may be sure that his preference is not founded upon fancy, yet may find it hard to state in words upon just what special characteristics he does base his choice. In the paper last year the names of makers were carefully excluded; this time it is impossible to get along without reference by name to the manufacturers of the instruments cited. I heartily wish it could be avoided, and accomplish the purpose intended, for it is a source of embarrassment to myself and is also liable to be seriously misinterpreted. All that can be said in justification of what follows is that I am under obligations to no one, either directly or by implication, except as necessitated by truth and fair dealing, and that all matters of personal interest are thoroughly placed aside, if I am capable of so doing. The articles used are all owned by myself or by the institution in whose service I am, with one somewhat conspicuous exception, and that was loaned me, upon request, for the purpose of this paper. No comparison is made with such as I have not had an abundant opportunity to test, and, with the exception just mentioned, with none that have not been in use during some years of time.

In the paper upon stands, a note was made upon the fact that we are prone to like best that with which we become accustomed. In the case of objectives, however, there is less room for such preference, because the mere handling of one is practically that of others, including the position and movements of one's body when at work. To be sure, in order to get the very best results with a high quality objective, one must patiently learn how to use that particular instrument, but this is another thing. The force of habit has little to do in this last case, while it is exceedingly strong in the method of moving the object under the lens, and in the manipulations generally of the stand.

It should also be stated that my work has chiefly been upon uncolored objects mounted in water, with or without the addition of carbolic acid or glycerine, and upon colored objects in balsam. The main exception is that of diatoms in balsam, and in this case as a test for the objective rather than work upon the objects for their own sake.

Magnification.

Whatever may be the facts in regard to the use of high-power eyepieces to secure the requisite magnification in mere tests, for long-continued work over the tube anything in the upper end of less than about one inch focal length is unsatisfactory to me. The strain upon the eye is certainly less with the medium and low power oculars, and the image is better to my eye, even with the finest objectives made. I choose, therefore, such focal length in the objective as will give sufficient magnification with a Huyghenian eye-piece amplifying about ten times, as the upper limit. Higher magnification by the eye-piece may be useful in testing an objective, and may, it is true, to some persons be available for long-continued work; but I am making a report of personal experience. The only other thing necessary to say here is that usually the less amplification the better, after a suitable amount is obtained. Hence neither objective nor eye-piece should be of less focal length than will conveniently serve the purpose required. For botanical laboratory use, a one-half inch and a one-fifth inch dry objective is the best selection for the common work of students. Occasionally higher powers are needed, sometimes running up to the highest and best procurable. For these exceptional cases provision should be made by having a few such objectives at hand, but students need not be furnished with them to keep in their desks, as with those first named.

Really serviceable magnification seems to reach its limit in about a one-fifteenth inch, or, at most, an one-eighteenth inch objective. Only in rare cases is anything of higher power than a one-tenth inch of best quality effectively superseded—with me in nothing but certain studies upon bacteria.

Angle of Aperture.

It appears to me that something similar can be said of the angle of aperture. In the matter of difficult resolution with oblique light, high class and even medium grade objectives have been, in my hands, proportionally successful in just about the order of their aperture, though exceptions have been noted ; but for most other uses it does not appear that the angle of aperture should be relatively rated so high in the qualities of objectives. It must not be inferred from this that wide angles are, in and of themselves, injurious for biological work. Other things being equal, I should always prefer them, cheerfully putting up with any lack of penetration, and to a certain extent with inconvenient working distance for the other advantages offered ; but crispness of outline, of even the smallest bacteria, depends upon other things quite as much as upon the aperture and cost-price of an objective. These smallest bacteria measure about $\frac{1}{2}$ μ ($\frac{1}{50000}$ inch), or about the distance apart of the dots from center to center of Pleurosigma angulatum. We all know that great angle is not necessary upon objects of this size. The question is whether excess of angle above a certain essential degree is of any importance whatever, or indeed whether an objective of wide aperture is on this account especially superior when the illumination is a narrow beam of axial light. When the object is too small or too slender to be seen by an objective of narrower angle, no doubt can exist even in this last case of the essential advantage of the greater aperture ; but unless one wishes to see the flagellum of a Bacillus or the minute structure of a diatom valve, his laboratory work may perhaps be just as successful with first-class objectives of less than the widest angle procurable. Should those of moderate angle possess better definition (not resolution), then for their proper work they are the better lenses. My later purchases for students' ordinary use have been of 110° air angle for a one-fifth inch, with the expectation that anything up to the widest numerical aperture may sometimes be accessible. For the closest possible studies upon the exact size (measurement and shape of small stained bacteria) a Tolles one-fifteenth inch homogeneous immersion of 123° balsam angle is the best I have used, though others at hand have considerably wider aperture.

Get the Best.

Having decided what is most suitable for the work proposed, the very best should be selected for students' use, as well as for special investigations. It may be said that the expense would often be too great, and that cheap instruments or none constitute the alternatives. Often, however, this is a mere outgrowth of too cheap ideas, either on the part of instructors or boards of trustees.

If the real needs are fairly appreciated, in this as in any other case, they can usually be met in some way; otherwise, how are microscopes obtained at all? At any rate, instructors should inform themselves with the utmost care, and then equip their pupils in the best possible manner with this the most delicate of all tools. No questions of home or foreign manufacture, of accidents of popular approval or of hereditary service, should be allowed weight in the selection of a microscope objective; neither should the cost price be taken as an index of quality. No one can be blamed for buying what he finds to be the best goods for the least money.

Governed by these principles, I have ceased ordering from abroad for students' use. Without naming other makers, I choose the objectives of the Bausch and Lomb Optical Company in preference to those of Leitz. I have in daily use some first-class wide-angled dry objectives of the Gundlach Optical Company that have given most excellent satisfaction. Anxious to have the best, as improvements were announced, I have ordered from time to time five first-class wide-angled objectives, each one supposed at the time to be the very best in the market. This paper may seem less presumptuous with this statement inserted.

Specific Tests.

I am now to report the results of some comparative tests made with certain named objectives under described methods of procedure. When the title of the paper was announced I hoped to have photographs taken in different ways for each objective tried, but have found too much time consumed in other directions to permit it this season. Please permit me to express the conviction that these proposed photographs would have certainly corroborated the statements herein made.

In order to decide with certain correctness of the relative quality of the objectives compared, the tests were purposely made as difficult as circumstances permitted, but under these difficulties each was given the best handling possible for the manipulation. The objectives, all homogeneous immersion, and that of Zeiss an apochromatic, were as follows :

Tolles' 1-15th (1880) balsam angle, 1.23° ; Zeiss' apochromatic 1-12th (1887) N. A., 1.40 ; Herbert Spencer's 1-10th (1888) balsam angle, 1.30° ; Gundlach's 1-12th (1890) balsam angle, 1.36°. The last was asked for and loaned to me for trial. An attempt was also made to include a Leitz's 1-12th (1888) N. A., 1.25°, but it was not possible to use it on the same stand ; hence not certainly under same conditions, and so it is not included in this report. I have not been able under fairly similar conditions to make it do what is reported for the others.

The tests were made upon Möller's balsam-mounted test plate, with an ordinary small coal-oil lamp with flat wick three-fifths inch wide and common round chimney, on which, however, was placed a tin extension 16 inches long to improve the combustion and steady the flame. Any one who sits down to a prolonged task of this kind will at least appreciate the latter improvement thus obtained. The lamp was placed 30 inches from mirror to the left, with center of flame, used edgewise, and mirror of the same height. The mirror bar was placed at the angle of 50° from axis of the instrument, and the concave mirror was accurately focused by means of the paper label on the test slide. No sub-stage helps of any kind were used. After adjustment of object, objective, and light, as described, an ordinary bull's-eye, same height of flame and mirror, was pushed in and out at will near the lamp, flat surface to the latter. The tube of the instrument could be closed to 6.5 inches, measured from its lower end to top of draw-tube, and could be elongated to 13 inches. The Tolles and Spencer objectives have screw collars. These were adjusted for their best effect with tube length of 10 inches, measured from front of objective ; for the others the tube was varied to suit. It should be said that the Zeiss objective was ordered for the long tube.

Amphipleura pellucida on this particular slide is of medium grade as to difficulty of resolution, but as difficult as any I have seen in Möller's test slides. Number 19 is probably proportionally easier, often showing by light and adjustments which 18 defies. I think this last is unusually difficult, and the same may be said of No. 12, *Grammatophora subtilissima*. The others seem to be fair average shells. As immersion media, somewhat thickened cedar oil, furnished by Zeiss, and a fluid sent out by Gundlach Optical Company, were used successively with all the objectives, with, however, no perceptible difference in result. The work was done in the daytime, with uncurtained windows behind operator. There were no windows in front or at the sides.

Under these conditions all four objectives resolved Amphipleura so plain that any tyro could make out the transverse lines, at least when the bulls-eye aided the illumination. Often the lines appeared the moment the focus was secured, and this could be changed back and forth with almost certainty that they would be evident whenever the proper adjustment was made. I need not say, however, that it always required careful work and that there were failures as well as triumphs. The two non-adjustable objectives did best with the shortest tube and negative ocular. With Zeiss' compensating ocular the result was rather more satisfactory with the ten-inch tube length. There did not, however, appear to be the same difference with the Gundlach in this respect, the Zeiss eye-piece also showing well with short tube. With the apochromatic at its best the diatom appeared perfectly flat, with midrib and margins showing distinct and clear when the lines were in focus, a thing not to be said of the others, though the Spencer came nearest to it when adjusted at 8, with ten-inch tube. The whole field, too, of the first named, including the object, was beautifully white. With the Gundlach it seemed to me that the lines were as distinct and crisp as with the Zeiss, and could be counted with reliability, a few at a time. When these were best shown the raphe and margins glowed with red, shading to dark, and a little movement of the focus downward was necessary to render the margins most distinct. With a longer tube the lines more evidently stood above the outline. With the Spencer at its best, I found little changes of illumination, etc., destroyed the resolution to a more marked degree than with the two others just named, and, though the lines were beautifully shown and the outline fair at same time, it seemed to me that counting would be a much more difficult undertaking. It should be remembered, however, that the magnification was less, and this I could not fairly make up with higher eye-piecing. Under a solid ¼th-inch ocular I was unable to make any distinction in the quality of the lines. With all the objectives, they were like parallel ropes, with uneven and woolly outlines.

The Tolles objective gave the lines readily enough, but partaking somewhat of the character just described with the other objectives and the solid ocular. With the magnification reduced to that of the 1-12th by the use of longer eye-pieces, the haziness of lines partially disappeared, but in no way did they seem so beautifully sharp as in the other cases. With both the Spencer and the Tolles there was a tinge of red in the raphe, in some cases merging into a dark shade, when the lines showed best under the manipulation of the screw collar.

Upon the whole, it seemed to me the apochromatic in this special test was really in the lead, though the distinction had to be carefully drawn. The results on other diatoms on the plate were similar, so far as could be determined, the rating of the objectives remaining the same.

I next tried the mirror in exactly central position, with other things remaining the same, save as the height and position of the lamp and bull's-eye required changing. This was varied, too, during same test by inserting a narrow-angled 1-5th-inch dry objective as a condenser, taking great care that it was in central position. In each case (to be further assured that the illumination was axial) examination was made by removing ocular and looking at the bright spot in the back lens of the objective. The difference in the performance of the objectives was certainly less marked than with oblique light. The required tube lengths remained about as before stated, with, however, less noticeable difference in a given amount of change. I obtained a kind of a glimmer of resolution on No. 19 with the apochromatic and Gundlach's lenses, but nothing with any of them on 18 or 20.

The others were well resolved by all four objectives, *Grammatophora subtilissima* giving the most trouble. I have never seen a balsam-mounted Amphipleura resolved by truly central illumination, though others have reported it with several objectives. When using the condenser named, by moving it only a little to one side, lines could be made out, but no comparative tests of this kind were made.

I had previously tried, with the help of an expert assistant, the three objectives in my possession in photographing violet-stained bacteria with central light, showing scarcely appreciable differences, but favoring the Zeiss and Spencer over the Tolles, unless the increased difficulties with the higher power proved too much for the skill of the manipulators.

A few of these results are herewith submitted for your inspection. All were balsam-mounted objects, and all were violet stains. Eastman's orthoscopic plates were used with a tuemeric screen. Some of these objects were among the hardest of the kind to catch by the camera, as a comparison with others will show.

I have now to add a word in regard to the durability of the apochromatic, the want of which has been frequently questioned After about two years' use it became evident that this lens was in some way impaired, and by looking through it from the back with a magnifier a hazy-granular appearance was noticeable, not due to dust on the back lens. Last March the objective was sent to the makers for examination and repair. It reached me again in July as good as new, with the statement that the front lens had been slightly decentered, and that the repair had been easily made, and was without charge. I have no other information upon this point, neither do I know what interpretation to place upon the granular appearance noted. There is certainly nothing of the kind visible now.

THE FULL UTILIZATION OF THE CAPACITY OF THE MICROSCOPE AND MEANS FOR OBTAINING THE SAME.

EDWARD BAUSCH.

The cover glass may truly be called a necessary evil; for, while absolutely required in microscopical investigations, there is no adjunct to the microscope that has been and is productive of so much evil, and has retarded the utilization of benefits made possible by the advance in the construction of objectives so much as it. This fact was appreciated as early as 1837, when the angular apertures were what would now be considered extremely limited, and the appreciable effect of variations in thickness of cover glass was not then nearly so pronounced as it is at the present time, even in modern objectives of a narrow angle.

The accommodation for the different thickness was obtained by varying the distance between the systems of objectives, and has been followed with modifications in the mode of obtaining the necessary motion up to the present day. While open to some objection, it accomplishes the purpose quite satisfactorily and must continue to be used until something better is suggested.

One of the purposes of the homogeneous immersion is, as we know, the avoidance of the necessity of the cover correction, in that the cover glass immersion fluid and front of objectives are to be one homogeneous mass; but even under these conditions, which in practice were found to be not constant, it has been found advisable to provide cover-correction to obtain the highest possible results. However, even should this not be found necessary in the development of improvements in this class of objectives, it must be remembered that the majority of objectives will always be dry, and especially so when such improvements, which we hope are still to be made, are accomplished. It is an unfortunate circumstance that with this class of objectives the influence of variation in thickness of cover glasses is most apparent; but since it is so, we should, if possible, provide an agency which, eliminating the personal factor of efficiency, will give, under all conditions, results closely equal to those under which the objectives were originally corrected.

It is surprising to see how little attention is paid to this subject in the large majority of standard works on the microscope. Almost all books give carefully prepared illustrations and descriptions showing the effect on the course of light by the interposition of the cover glass, and after giving conclusive evidence of its disturbing influence, still, in a general way, say it is of little moment. Thus, in a German work of the highest standing, which has also been translated into the English language, is found the following utterance, freely translated:

"In regard to modern microscopes, which we have had opportunity to examine, we have not found the difference in thickness such as occurs in commercial cover glass, when, for instance, three to six are equal to a mm., has any noticeable influence on the microscopical image."

In another work of great popularity are found the following quotations: "That the effect of thickness of cover glass has a great influence on the perfection of the microscopical image is beyond the slightest question, and certainly deserves the most careful attention of the optician as well as the observer; but whether the devices of its removal are of such great importance and so absolutely necessary as it is claimed, is another question. On the other side, the difference in the cover glass used in different directions for the most delicate preparations is hardly of any account. I at least possess, besides my individual preparations covered with glass of about 1–5 mm. thickness, a collection of objects which I obtained from London and Paris, in which there is such a slight difference of cover-glass thickness that I can observe them all with my objectives of powers from 2.0 to 1.3 mm. (equivalent to about 1–12 to 1–20 inch) without showing the slightest difference in optical qualities and in the definition and clearness of the image under the same illumination, as I have convinced myself by careful comparative tests."

With such statements to guide the microscopists, it is not surprising that the subject should have received so little attention, and that any efforts to lead to improved methods of manipulating objectives should have almost completely failed because of a lack of the true understanding of their need and consequent failure to create interest. The belief is quite general that any time devoted to this subject is wasted and might better be utilized in other directions.

I hope to be able to show that this is entirely wrong, and may here say that, while I may be considered an extremist in the other direction, my efforts emanate from the desire to put it in the power of every microscopist to obtain the highest possible results from his optical battery and equal to those obtainable by the optician.

When in 1887 Prof. S. H. Gage addressed a circular letter to all opticians in the world inquiring for the dimensions of their standard tube length, as well as for the thickness of cover glass which they used as a standard in the correction of objectives, I looked forward to the result with considerable interest, as it would bring together data which it was impossible to obtain otherwise

At the meeting of this society in 1887, at Pittsburgh, he gave the results of his efforts, which show some astonishing facts. I would here say that while for a long time I had felt that a system that would permit the full utilization of the optical capacity of objectives of different makers under varying conditions of cover glasses was desirable, I was then forcibly impressed with the absolute necessity of a plan which would offer this advantage. Outside of the differences of the lengths of tubes used by different makers, which is also of great bearing on the spherical correction of objectives, one is astounded by the difference in standard cover glasses used by different makers in correcting non-adjustable objectives. With a thickness of 0.10 mm. for the thinnest and 0.25 mm. for the thickest, it is only too apparent that with the additional variation in lengths of tubes it is beyond the power of the microscopist to obtain even approximately the best results from his objectives. More than this, a large quota of the advance made in recent years in the capacity of objectives has been lost.

As Professor Gage states, "A uniform thickness for cover glass for non-adjustable objectives seems also desirable," and this would be the easiest solution of the question ; but while on the one hand the makers of objectives have not yet agreed to use one standard on account of the technical difficulties involved in departing from their established precedent, on the other the microscopist would hardly be willing to bear the expense which would be occasioned by the loss of cover glass not conforming to the standard in order to use those of one thickness. This expense might be greatly reduced by using selected covers of one standard on objects for all medium and high power objectives and the balance on all other preparations on which only low powers would be used, but this would of course be of little avail in face of the fact that manufacturers follow no standard.

The greatest difficulty is met with non-adjustable objectives. As is well known, compensation for thickness may be obtained in the proper adjustment of tube length; but while not all microscopes are suitably provided with draw-tubes, the requisite experience and skill is lacking with a large number of microscopists to properly make the correction in this manner, as well as in objectives specially provided with collar correction. I am sure that microscopists of long experience will bear me out in the statement that results with adjustable objectives depend upon individual skill, and that many such objectives now in use fail to give results corresponding to their capacity. It would seem, therefore, that any system to permit the full utilization of the capacity of objectives should depend on no personal factor—in fact, should be mechanical—and this I have followed out in the system I shall explain.

In an objective corrected for normal thickness of cover glass there will be spherical over-correction with thick covers and under-correction with thin covers, the amount of correction varying in a different ratio to the amount of variations from the normal thickness. The chromatic correction will also lose correspondingly, but to not so high a degree. While a deviation of a few hundred millimeters in either direction will, perhaps, not signify, that which occurs in covers classified in price-list under one number is sufficient to seriously affect and the high powers totally obliterate the definition which under normal conditions it may possess. The microscopist is therefore not obtaining such results as his objectives ought to enable him to obtain, and the efforts of the conscientious optician to provide classified objectives of reliability and similar performance is almost entirely nullified. In making the necessary experiments some astonishing results appear. With a non-adjustable dry 1–5 corrected for a cover glass of 0.16 mm., employing the extremes of cover glass which are used by the various manufacturers as standard, as obtained by the efforts of Prof. Gage, I found that for 0.25 mm. a tube length of 6 inches is required to obtain the proper correction, while for a thickness of 0.10 mm. 13 inches of tube length is necessary. In a 1–8 objective adjusted under the same conditions 4½ inches is the requisite for a cover of 0.25 mm., and for 0.10 mm. 15 inches. The further fact is shown that with a 1–5, which, under conditions of tube length and cover glass given above, shows certain structure, well defined, absolutely fails to show anything of it under a cover glass of 0.10 mm. on one side, and 0.25 mm. on the other, and further a marked chromatic over or under correction.

With a cover of 0.14, which would seem but a slight variation from the standard, the objective is spherically highly under-corrected, and with 0.18, highly over-corrected. With objectives of high power the difference is still more marked. For these experiments I have had Mr. J. D. Moeller, of Germany, mount a series of Pleurosigma angulatum dry and Amphipleura pellucida in balsam, under a series of covers varying from 0.10 mm. to 0.34 mm., each carefully measured and marked. I have used these objects because they are my favorite tests, and it goes without argument in saying that any preparation showing structure under above objectives will be affected to the same extent by the varying conditions of cover glass, as these objects and in objects of still finer structure the limit of visibility will be reached correspondingly sooner.

The system which I have devised to aid in overcoming these difficulties depends in the first instance upon a micrometer for measuring the thickness of cover glass. While the delicate instruments made by M. Grossman, of Germany, are excellently suited for this purpose, they are expensive. I have endeavored to overcome this objection by constructing a plain screw which, while not so sensitive to the touch, is sufficiently so for all practical purposes. The instrument is provided with a stand of japanned iron. Cut horizontally through the top is a thread of $\frac{1}{50}$-inch pitch and $\frac{3}{16}$-inch outside diameter. A recess is cut on the top below the line of the screw and at right angles to it for placing the covers. The one-half of the top of the stand which receives the micrometer screw is slotted longitudinally to the depth of the screw and is provided with a set-screw to take up wear. The other half has the fixed screw, adjustable, however, for final adjustment. The end of the micrometer screw is milled, but of a small diameter, so that no force can be exerted so as to endanger the cover glass. Fixed on the screw between two nuts is a brass drum with a ½-inch face. A knife-edge index finger is fixed to the top of the stand and projects over the top of the drum. To the outside diameter of the drum is fixed a strip of glazed paper provided with a series of divisions. The first gives the thickness of cover glass in one-thousandth inches, the second one-hundredth millimeter. The third indicates the proper tube length with various thickness of cover glass with a non-adjustable ¼ corrected under a tube length of 8½ inches and cover thickness of 0.16 mm.; the fourth gives the tube lengths of a 1-5 inch objective under the same conditions; the fifth for a ⅛, and the sixth for a 1-12 for same conditions of tube length and cover;

the seventh is for a ⅙ with the same cover and tube length of 160.0 mm.

In objectives provided with cover correction the graduation is so arranged as to read to 1–100 mm. No matter what the power of objective or whether dry or water immersion, the number gives proper correction for a thickness corresponding to it. Thus, with a cover glass of 0.20 mm. the collar of such an objective need merely to be set at 20 to give the proper correction and, consequently, the best results. On the other hand, with an objective which is graduated on this system the correct thickness of cover glasses can be determined by obtaining the proper correction on preparations previously made, but on which the thickness of cover glass is not noted and the thickness may be marked on them for future convenience. To do this successfully, however, the necessary experience is requisite. All the other scales give the correct tube length in inches and millimeter for covers corresponding to them, and in this manner offer a ready and definite means of correction. The tube lengths required for the thinnest and thickest covers are so extreme that probably no convenient means for obtaining them can be practically arranged, but they can be so approximately if not entirely. At any rate, the micrometer will detect the requirements before using the covers, and those deviating considerably from the normal can be used on objects for use with low powers only, in which case the effect will not be very appreciable.

In this system I do not overlook the fact that variation in tube length involves a variation in magnifying power; but, except in cases when micrometers are used, I consider this of secondary importance, as it always is in comparison to results obtained in resolving and defining power.

This system involves four conditions:

First. That all cover glass be measured before using them, and that the thickness be noted on the preparation.

Second. That for convenience all draw-tubes be marked in inches or millimeters or both.

Third. That adjustable objectives be corrected according to this scale.

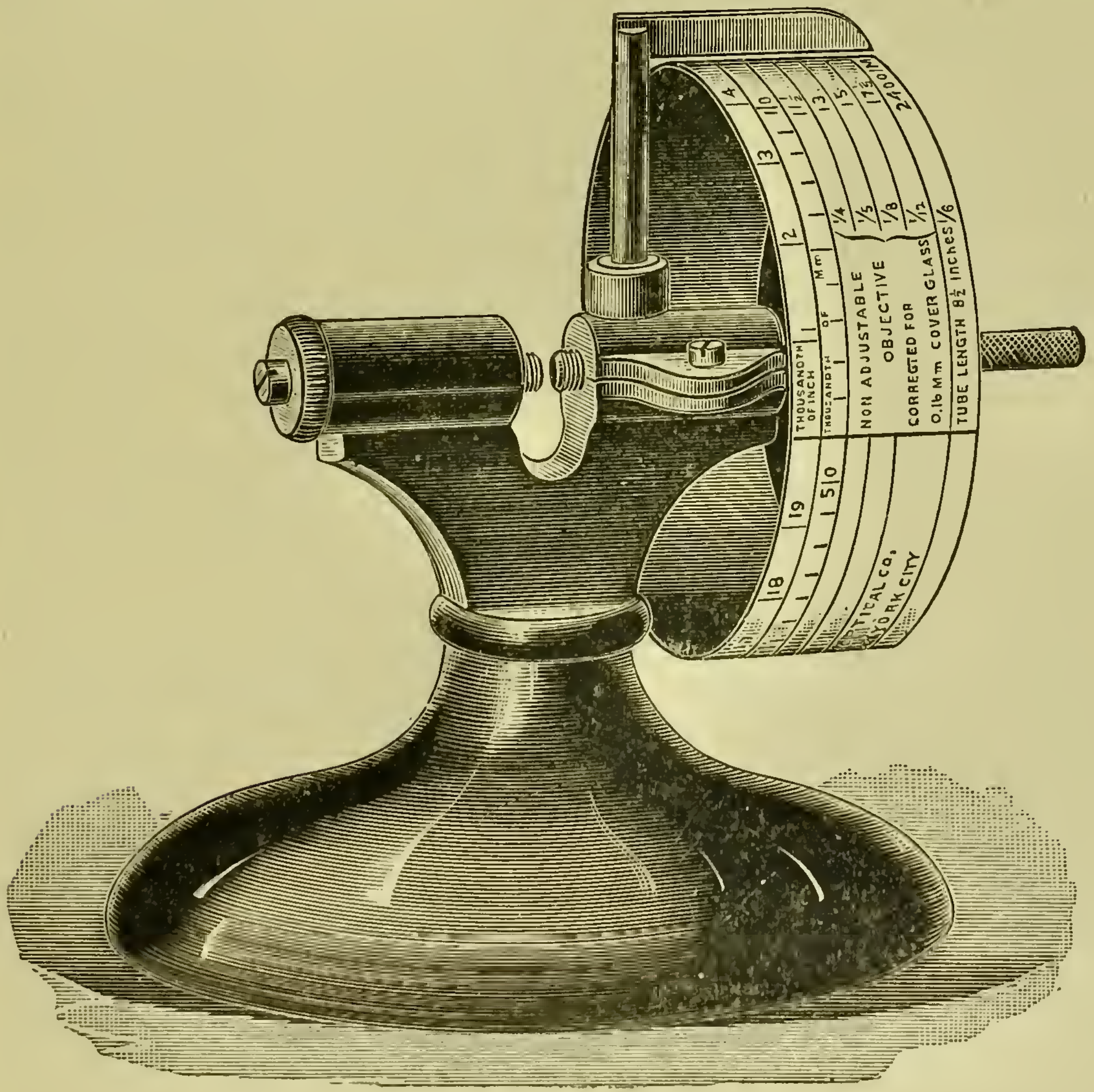
Fourth. That the same tube length and cover glass thickness be used in all original corrections of objectives.

As regards the first condition, there are many microscopists now who measure all their covers before using them; but the mere knowledge of thickness has been of no value up to the present time, because this in itself has been no guide in obtaining better results except by approximation. My aim in connection with this system has been to devise an instrument which possesses a high degree of accuracy and still be so inexpensive that its price should be no obstacle to its general use.

The celebrated preparer of objects, Mr. J. D. Moeller, and others have kindly agreed to mark the thickness of covers in their objects so as to aid the introduction of this system, and other preparers can no doubt be induced to do so if its advantages can be proven.

As regards the second point, many manufacturers now graduate their tubes, and modern requirements demand that this should be more generally done. Our company intends, as soon as it can possibly arrange to do so, to graduate the tubes of all its instruments.

As to the third and fourth conditions, I cannot, of course, presume to ask manufacturers to adapt their standards to this system. While it will be a convenience to a large number of microscopists, I must leave it to the merits this system may possess to exert their influence in this direction.



ON A MOOTED MATTER IN THE USE OF AN EYE-PIECE IN PHOTOMICROGRAPHY.

A. Clifford Mercer, M. D., F. R. M. S.

At last year's meeting of this Society, the writer opposed the ordinary use of the eye-piece in high-power photomicrography. He held that a sensitive objective nicely adjusted for cover thickness and focused cannot suffer a change in position without having the nicety of its adjustment to some extent vitiated. The ordinary use of the eye-piece in photomicrography involves such a change in position and therefore vitiates to a corresponding degree the resulting image.

Let us see how such a change is so involved. Rays of light leaving a point in an object in focus and entering the microscope pass out of the eye-piece divergent or parallel to enable the normal eye to focus them on the retina. These rays must be divergent or parallel, because only such rays are focused on the retina by a normal eye. Now, as divergent or parallel rays cannot form a real image, the microscope under the foregoing conditions does not project an image on a screen held anywhere above the eye-piece. To get an image above the eye-piece, the divergent or parallel rays are in ordinary practice made convergent, and therefore image-forming, by focusing; and it is this procedure which changes the position of the objective.

On the other hand, Dr. Blackham said, in reply, that when an object is in focus for a normal eye looking through the microscope, a plane can be found somewhere above the eye-piece in which a real image of the object is formed. A second focusing is therefore unnecessary; and the adjustment of the objective is not disturbed. A few members reported that they had secured photomicrographs in this plane. Dr. Blackham undertook to demonstrate the formation of the image by means of a solar microscope in an adjoining room. The prescribed conditions seemed to be met; and, apparently, an image was formed on a screen about ten inches from the eye-piece.

Dr. Blackham's demonstration was accepted as satisfactory and conclusive. He was, however, unable to give, when requested, a diagramatic or theoretical explanation of the formation of the image. Another member had puzzled over the apparent conflict between observation and theory during two years without finding a solution. The chief purpose of this paper is to record a series of experiments by the repetition of which any sufficiently interested microscopist can satisfy himself that Dr. Blackham's demonstration was in some way at fault, that a real image cannot be formed under the prescribed conditions and that the failure to theoretically explain the formation of an image under those conditions was a proper and to be expected result.

Experiment 1. The rulings of a stage micrometer, under an inch-and-a-half objective, were focused for a normal eye looking through a horizontally arranged microscope. A screen was placed in contact with the eye-piece and slowly removed to a distance of five feet without finding an image in any plane through which it passed. But so soon as the microscope was racked slightly away from the object, a sharp image appeared on the screen; and, on again looking through the microscope, the lines seen at first had disappeared.

Experiment 2. The second experiment was a repetition of the first, excepting that, instead of micrometer rulings approximately lying in a single plane, the object was a somewhat thick section of lung tissue. When the object was in focus for a normal eye looking through the microscope, a blurred image could be seen on the screen. On racking the microscope away from the object, the image on the screen became sharp, while the object as seen through the instrument lost its sharpness. In the former instance the sharp focus was found to be on the nearer surface of the object, while the blurred image was of a deeper plane; and in the second instance *vice versa*.

Experiment 3. The dust on the cover glass of a mount, under an inch-and-a-half objective, was focused for a normal eye looking through the microscope. On removing the head, an image of the object beneath the cover glass appeared on the screen about ten inches from the eye-piece.

Experiment 4. A nummulite, under a three-inch objective and two-inch eye-piece, was focused for a normal eye looking through the microscope. With this arrangement a sensitized plate was exposed at ten inches from the eye-piece. Photomicrograph 1 is a print from the resulting negative. The result is as good as could be obtained at any distance from the eye-piece under the prescribed conditions. The microscope was then racked away from the object until a sharp image appeared at the ten-inch distance. A second sensitized plate was exposed. Photomicrograph 2 is a print from the resulting negative. (Photomicrographs 1 and 2 were shown at the meeting. 1 was not in focus. 2 was sharp.)

Experiment 5. A physician, 68 years old, with presbyopic eyes and long accustomed to the use of a microscope, focused, without his spectacles, an object as he saw it through the eye-piece. On removing his head, an image of the object appeared on a screen about twenty-seven inches from the eye-piece. A normal eye looking through the microscope could not see the object.

Experiment 6. An Abbe test-plate, under an inch-and-a-half objective, was focused for an eye made, as it were, temporarily hypermetropic by wearing a six-inch negative spectacle lens. On removing the head, a sharp image appeared on a screen about ten inches from the eye-piece. The normal eye without the spectacle lens could not see the object on again looking through the microscope.

Experiment 7. Delicate imperfections in the lines of an Abbe test-plate, under a Zeiss ⅛-inch objective and a two-inch eye-piece on a horizontally arranged No. 3 Powell and Lealand stand, were focused for a normal eye looking through the microscope. A sharp real image could not be seen in any plane within six feet of the eye-piece. A four-drachm weight was carefully placed on the cap of the eye-piece, when a sharp image of the imperfections appeared on the screen about fifteen inches from the eye-piece. On again looking through the microscope, the imperfections of the virtual image had lost their sharpness. The weight acted on the eye end of the microscope tube as on a lever, and thus slightly tipped up the objective and increased the distance between it and the object.

Experiment 8. The dark dots on a light ground of *pleurosigma angulatum*, under a Powell and Lealand 1–12-inch apochromatic and a one-inch compensating eye-piece on a horizontally arranged No. 3 Powell and Lealand stand, were focused for a normal eye looking through the microscope. On removing the head, a blurred image appeared on the screen about fifteen inches from the eye-piece; but this blurred image presented the reverse appearance of light dots on a dark ground. A one-drachm weight was carefully placed on the cap of the eye-piece, when a sharp image of the dark dots on a light ground appeared on the screen. On again looking through the microscope the dark dots had lost their sharpness. By removing the weight the original conditions were restored. The sharp black dots were once more seen through the microscope, and only blurred white dots on the screen.

Inductions from these experiments are: First, when the microscope is focused for a normal eye on an object approximately a single plane, the instrument does not project a real image of the object above the eye-piece; and, secondly, when the microscope is so arranged as to project a real image of a very thin object on a screen above the eye-piece, the object is not in focus for a normal eye looking through the instrument. These inductions are in harmony with theoretical optics. Observations really opposed to such inductions must fail to find supporting explanations in theoretical optics.

The announced chief purpose of this paper, to record a series of experiments for particular ends, has been attained. A secondary purpose is to call attention to the fact that the recorded experiments also at least suggest explanations of the apparently opposed phenomena brought to the notice of the Society a year ago.

Let us consider Dr. Blackham's apparently contradictory demonstration. The object was a section of animal tissue showing well-injected blood vessels, and therefore necessarily somewhat thick. A normal eye looking through a microscope might focus the nearer surface of such a section and, on removing the head, find a real image of a deeper plane projected on a screen above or beyond the eye-piece. In this instance the virtual and real images would be in a general way alike, and might be carelessly considered images of one and the same plane of the object. I believe those of us who were present and had glimpses of Dr. Blackham's virtual and real images were careless in the way suggested.

In regard to the fact that some members had focused an object looking through the microscope and then, without changing the conditions of the instrument, had secured a photomicrograph of the object beyond the eye-piece, the following suggestions are offered in explanation: The virtual focusing may have been on the nearer surface of the object and the photomicrograph a picture of a deeper plane; or the observer's eye may have been presbyopic or otherwise hypermetropic; or, in the case of high-power work, in making a connection between the microscope and camera a little weight on the eye end of the microscope tube may have made the necessary alteration in focus; or a virtual image may have been focused with an erect tube when the weight of the eye-piece tends to press the objective toward the object and the photomicrograph secured with a horizontal tube when the weight of the eye-piece tends to tip the objective up and away from the object; or, and finally, in the case of exceptionally faulty correction the paths of the visual rays may have been so different from the paths of the actinic rays that the former may have given a virtual image and the latter a real image under the same conditions of the microscope.

Discussion of Dr. Mercer's Paper by J. D. Cox.

(*See Minutes of Proceedings.*)

The subject of photographing with the eye-piece in the microscope was discussed at the Chautauqua meeting, and references to it made in subsequent meetings led him to think the contention of Dr. Mercer's paper, as well as some former papers, to be based upon a misconception of what the speaker said in the first meeting referred to.

In advocating the use of the eye-piece in photomicrography he had done so chiefly because of the convenience in manipulation, for in this way high amplifications can be secured with a very moderate extension of the camera bellows; but he had also urged that the microscope is a compound optical instrument in which the objective is corrected for use with the eye-piece, and the substitution of an amplifier for the ocular involves the risk of combining parts that were not made for each other and are more likely to prove ill-adapted to each other.

The experience of Dr. Woodward had been referred to; but Dr. W. had left it on record that the amplifiers he specially ordered from distinguished opticians failed to give satisfactory results, one made by Tolles being the exception, and that apparently by accident, since others by the same maker did not work well.

Some papers formerly read before the Society seemed to argue that in the very nature of the case no sharp image is or can be projected upon a screen when the eye-piece is used. To this the old refutation is offered, "*solvit eundo.*" Photographs of delicate objects made with use of eye-piece are produced and the image *is* sharp. Not to mention the speaker's own photographs of diatoms, Dr. Detmers had photographed *amphipleura pellucida* with the eye-piece by preference, and his prints challenged comparison with any ever made, as his were, by lamplight. Very recently Dr. Baker had photographed *pleurosigma angulatum* in a way to rival any results attained by anybody anywhere. It was done by lamp and with the eye-piece. The amplification in some of these instances reaches or exceeds 2,000 diameters. If the thing is thus done in fact, there must be some error in reasoning which concludes that it can't be done.

In listening attentively to Dr. Mercer, the speaker thought his reasoning proceeded on the supposition that the focusing of the microscope is not meddled with when the instrument is moved from the eye to the camera. It was astonishing that this should be assumed. Photographs have been exhibited and published of varying amplifications made with the same objective and eye-piece. The society cannot need to be reminded that these different amplifications imply different extensions of the camera bellows, and that for every change of such extension a change of focus of the microscope is necessary, on the fundamental and elementary principle that the lengthening of one of the conjugate foci of an optical lens or combination of lenses implies the shortening of the other.

If it was assumed that the focus used with the eye was retained in photographing with varying amplifications, it was a misconception which would be removed as soon as it was known to exist. The speaker certainly never dreamed that any one could suppose he had asserted that his work in photography was done in that way. The opponents of the use of the eye-piece have seemed to argue that it is theoretically impossible to produce an image on the screen by means of the whole microscope, objective and eye-piece combined. If they only meant to say that we cannot do so unless we focus the image properly on the screen, the debate has been almost ludicrously vain, as much so as to say that to see an object under the microscope with the eye we must focus for it and focus differently for different eyes.

In saying that the speaker used the microscope in photographing under the same conditions as in investigating, he meant that he used the whole compound instrument, objective and ocular, with the illumination the same in kind and general adjustment. In transferring the instrument to the camera the place of the lamp is usually changed and the illumination has to be re-adjusted, but a pivoted support for lamp and microscope may be so arranged that the light will remain precisely as used for the eye. The focus, as already said, must be re-adjusted till the image is sharp upon the screen at the amplification and consequent extension of camera bellows determined upon.

THE BINOCULAR MICROSCOPE OF THE SEVENTEENTH CENTURY.

Charles E. West.

It is generally understood, I believe, that the binocular microscope is a modern invention—that it goes no further back than the one made about 1853, by Prof. J. L. Riddell, of the University of Louisiana. His instrument was a rude affair, made of two pieces of looking glass, which was improved by Prof. A. K. Eaton, of this city, by substituting for the mirrors a solid prism of glass made of two triangular prisms which were cemented together. Riddell adopted the improvement. This is the basis of the modern binocular microscope. Its perfection depends upon the equal division of the beam of light by the prism, or the nearness of that division into halves, so that the same amount of light may traverse each of the tubes to the eyes.

I never used but one instrument that did this, and that was made by J. Zentmayer, of Philadelphia. I tried it on the P. *angulatum* with the 1–5, 1–10, 1–15, 1–30, and 1–40th objectives, which resolved the frustule as satisfactorily as with a monocular. I have never found another binocular of this maker that would do this, and I have tried several.

But the first inventor of the binocular seems to have been Antonius Maria de Rheita, a Bohemian Capuchin, mathematician and astronomer, who published a work in 1645, under the title of "Oculus Enoch et Eliæ, sive Radius Siderio Mysticus," a rare book, which I found in the Astor Library of New York.

By a contemporary writer, R. P. F. Ioanne Zahn, who published an Optical Treatise in 1685, entitled "Oculus Artificialis Teledioptricus, sive Teliscopium," there is given a minute description of de Rheita's binocular microscope and telescope. This work is in my possession, and from it I hope to prove that the binocular microscope is no new thing.

In a letter to his friend, J. Caramuelis, dated Cologne, 24 April, 1643, de Rheita speaks of having detected most clearly, by means of his binocular telescope, "with the greatest surprise, admiration, and delight, the sacred Sudarium Veronicae sive faciem Domini maxima similitudina in astris expressum," in the sign of Leo, between the equinoctial and zodiacal circles. Zahn has given in his work a figure of our Lord's head as pictured on the handkerchief of St. Veronica. George F. Chambers, in his revision of Admiral Smyth's "Cycle of Celestial Objects," has given a reproduction of the figure and characterized it a "Pious Fraud," A. D. 1643.

I propose to give a translation from the Latin of such parts of the "Oculus" as have a bearing upon the binocular microscope, as follows:

"Since the distance between the eyes is not the same for all persons, first of all an artificer must ascertain this distance if he wishes to construct a tube perfectly fitting any one person. This can be best ascertained (as elsewhere has been shown and as de Rheita also has said) by the aid of a pair of compasses and a mirror. This distance serves for adjusting the first ocular lenses, since in the binocular tube the two other lenses nearest the eye must be similarly placed to each other—*e. g.*, if the center of the pupils of any person be distant A B, 22–100 ped. Rom. (Fig. 1 of plate), at this distance also from each other must the other first lenses be, *a b* and *c d*, as is shown by the figure, so that the center of the lenses *a b* and *c d* should meet at the points A and B. *

"Let us suppose one simple tube be constructed with a convex ocular lens of very great aperture, which shall be greater than the distance of the two eyes, *generally speaking* it will be impossible, by using one convex ocular lens, however large, for an image radiating from the tube to affect both eyes alike, for if the image radiating from the tube through one lens could affect both eyes alike, both eyes must be at the point of contact; but this is impossible, since both eyes cannot be at the same point at the same time, and thus all parts and points of the image cannot be transmitted to the same parts and points of both eyes at once. I have said *generally speaking*, because if one look at a mirror of very obtuse convexity and at a great distance

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*The plate is reduced one-half from pencil tracings. The letters on Fig. 1 were accidentally omitted. A B are the ends of the straight line, *a b*, *b c* the diameters of the circles on the same line prolonged. The circles represent the eye lenses to be just as far apart as the observer's eyes.—W. H. S.

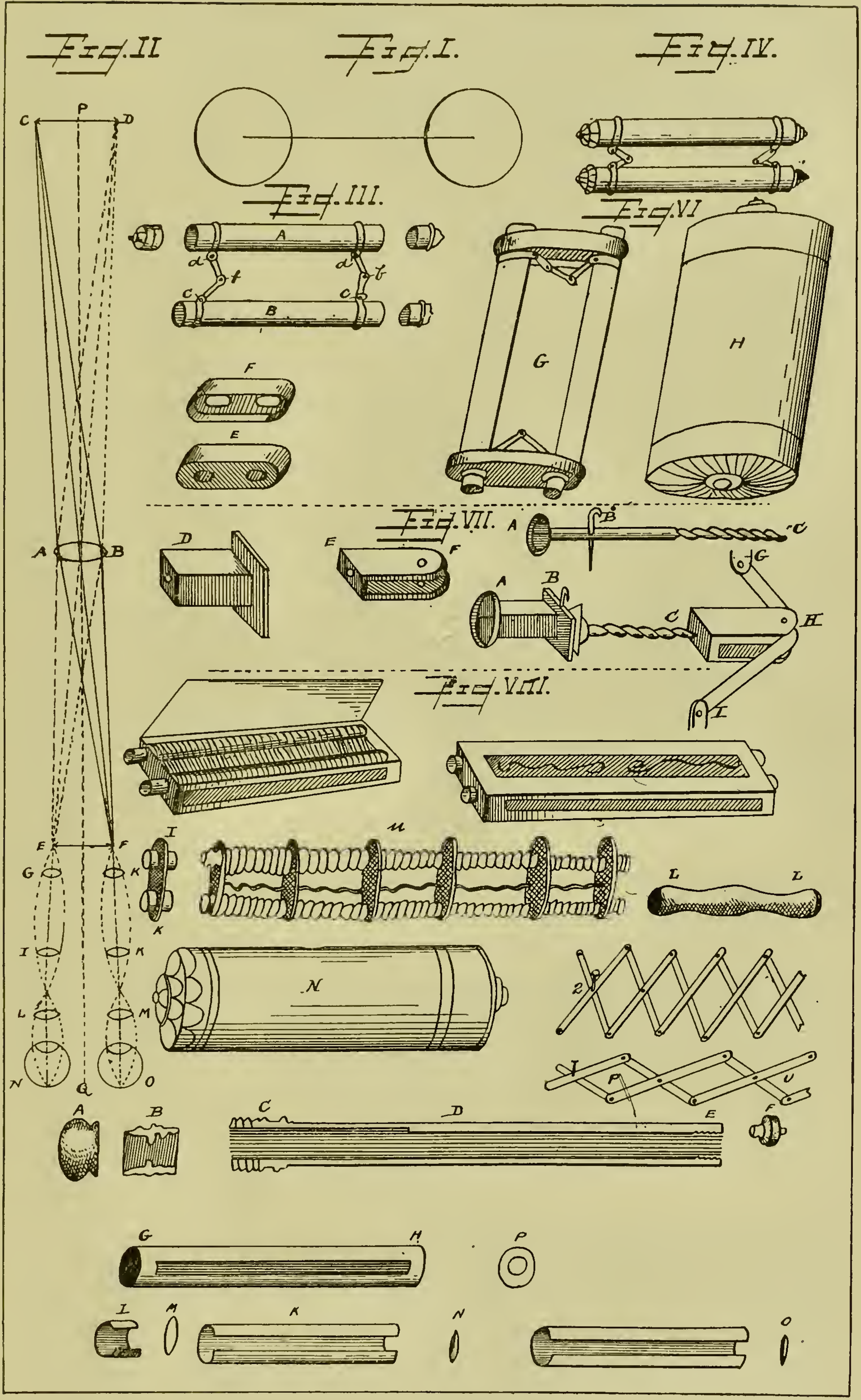
Fig. II
Fig. I
Fig. IV
Fig. III
Fig. VI
Fig. VII
Fig. VIII
C
P
D
A
B
E
F
G
K
I
L
M
N
O
Q
a
b
c
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H
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V
U

from the eyes, perhaps something can be effected; but it is of little practical use, for the objective lens must be of extremely long diameter to represent the object sufficiently large and near, so the tube would be obliged to be of enormous length."

III.

"A binocular tube can be constructed with one objective lens, when the image thrown from the objective lens can extend itself to a sufficient distance in the base of the divergence and thence again radiate through ocular lenses to both eyes. Thus the object C P D (Fig. 2), radiating through the objective lens, A B, forms the image E F; but the rays of the image, digressing from E, reach the eye N through the ocular lenses G I L, and from F reach the eye O through the lenses H K M. Thus a binocular tube can be constructed with one objective lens A B; but by this method the rays from the object reach the eye after being greatly refracted and diverging very far from the axis P Q of the objective lens, so that the image is never clear and distinct. Moreover, both eyes cannot see the whole of one object, nor is the same part of the object seen by both eyes, though more of the object can be seen by moving it about, but it will always be confused. Hence a tube thus constructed is never of much use."

IV.

"Even if one large lens be taken having two apertures at exactly the proper distance apart and both eyes look through these apertures at a single object, and thus see one and the same thing (which, however, seems impossible), nevertheless a perfect binocular tube cannot be formed, because just as the radiation from the object comes to both eyes by rays remote from the axis, and hence more refracted, so will the image possessed in the eye be indistinct and confused."

V.

"The best construction of the binocular tube is made by two telescopes exactly alike, so fitted to both eyes that the optical axis passes through one and the same object (Figs. 3, 4, 6, 7, and 8). The two objective lenses must be exactly alike; not only of the same shape, but also alike in magnifying power and in point of clearness. (The same care must be taken whether the lenses be concave or convex.) Again, the telescopes must have the same "ordinate," so that not only the objective lines but also the eyes similarly placed will correspond precisely to each other; but the telescopes and their first ocular lenses must be as far apart as is the distance between the centers of the pupils of the two eyes. The telescopes should be so directed toward one and the same object as if there were but one aperture for both tubes, and through this aperture the object, brought wonderfully near, can be very distinctly seen."

VI.

"Since there are various kinds of telescopes, the ordinary Galilean, made of convexo-concave lenses, others convexo-convex, which can be made from pure convex lenses, and, indeed, again, out of these others can be so formed that they present objects inverted, as it is said to be the case with astronomical telescopes. Others present the object erect, but whether straight or curving inwards depends on the nature of the ordinate. The former can be made from four convex lenses, the latter from a plane glass and two very powerful lenses, etc. Thus also various binocular tubes can be made, depending on the ordinates of the telescope, provided that the ordinates be the same for the same binocular tube, taking the distance of the first ocular lens and the direction of both ordinates through one aperture, so to speak, so that the same object will be seen single and not double."

VII.

"For lesser binocular tubes, common telescopes not exceeding a foot in length are best, because those more than a foot long do not present the object completely enough. The smaller the telescopes the greater the object. Tubes of this kind are made with three lenses for each telescope, having their ordinates placed as indicated in number 13, cap. 5, *seq.* The proportion of the lines there indicated is also much approved."

VIII.

"How to join the two telescopes: The two tubes may be made of copper or brass plates, as in figure 3, where two tubes, A and B, are joined by brass hinges at *a b c;* then, where the tubes are placed, both eyes are accommodated to any remote object, and the object will appear single and with great clearness. How the tubes may be joined, placed, closed, and covered is shown in figure 4. Likewise several movable rods similarly connected could be placed in each tube, but this method has disadvantages. Two tubes with their lenses arranged so as to be adapted to any vision are best constructed by cases or capsules as follows: Make a capsule of brass leaf in the form of an ellipse, as shown in H (Fig. 6). Make two apertures in the lesser capsules E and F as far apart as the distance between the eyes. These apertures must also be elliptical, as E and F in the figure. The size of the apertures should be such that the tubes or rods A and B, also C and D, would perfectly correspond.

"The two disks, E and F, of both tubes must be so firmly fastened that the tubes will not slide up and down too easily. Thus we now have all things arranged as in the figure G. This entire affair can be put into the greater capsule H and there encased. Thus it can be easily and safely carried about. The capsule H may be of wood, leather, or other material, and decorated to suit the taste."

IX.

"Since the distance between the two eyes does not differ much for many individuals, and also since the eyes easily accommodate themselves, many artificers fit their own eyes and fasten the rods in the tubes firmly in one position, so that they cannot be moved. I have made many such myself. Although they fit most people well enough, yet some eyes are unpleasantly affected after looking steadily for a long time. Least of all must such tubes be used for magnifying, unless they be adjusted to the distance between the eyes, since they harm the eyes not a little, and when used too much may even turn the eyes from their natural position (cross-eyed), as I have known happened to a certain nobleman. Thus it is always better to make the rods movable, so that they can be fitted to all eyes; yet it is allowable to make a little common binocular tube with movable rods, to be placed in a case after the manner of a little book, thus: Make a small capsule the size of the tubes which it is to contain, cover this with leather, and put on clasps just like a little book. Put two thin, wooden tablets at the ends where the ocular bands and objectives meet, but where the clasps are opened. These must be shoved aside, so that the eyes can look through the tubes. The cases may be so constructed that the open space between the two tubes can hold the "ignitabulum" with its sulphur thread (match box) and the burning-glass for quickly making a fire in the field or anywhere you please. Above this space a cover of thin brass is placed, on which is fixed a magnetic needle. At the other end, which is covered with leather where the book closes, can be placed a movable circle to point to the moon, according to the hour of the night; and thus various other things can be added."

X.

"The long rods holding the lenses can be of various materials and shapes, but it is best to be so made that the lenses can be cleaned when necessary, for they will become dusty, no matter how carefully they are closed. That part of the tubes just before and just behind each ocular or objective lens should be so arranged that a small, linen cloth could be inserted through an aperture for the purpose of wiping the lens."

XI.

"In convexo-convex binocular tubes it is better for the first ocular lenses to be quite acute, so that the eyes can be placed nearer the same objects. They should always be a little more acute at the bottom than in simple convexo-convex tubes, since there is a greater clearness from the two eyes looking at the same thing. I have found these to be good proportions: The objective lenses remove the focus to a distance of 1½ feet; the middle ocular lens, equally convex on each side, has a diameter 35–100 Rom. feet; the first ocular lenses anywhere from a diameter 20–100. This is also a good binocular tube. Objective lenses remove the focus to a distance of two feet; middle lenses, equally convex, 40-100 diameter; first lenses, near the eye, 20-100 and 25-100 diameter. Another good tube, objective lenses at a focus of four feet. Middle lenses, equally convex on all sides, 50–100 diam. First ocular lenses, unequally convex, from diameter 30–100 and 35–100."

XII.

"Two convexo-convex telescopes enclosed in a case can be made adjustable to any vision in the following manner: Join the two telescopes by movable arms (as indicated in number 8 of this chapter) and through these arms place a spiral screw, which can lengthen or shorten the telescope at will. This is shown in the 7th figure. A B C is a nail cylindrically round from A to B, spirally round from B to C. At B is a small nail, by which the large nail A B C is kept in place after being placed through the round aperture D. Thus the arms G H and I H, joining the two telescopes, can be contracted or extended. The form E F is placed upon H, and both arms, G H I, are held in place by a small nail passing through F and H. There the spiral part B C is placed through E. Now, if the head A of the nail A B C be turned this way or that, the arms G H and I H, joining the two telescopes, will be contracted or lengthened."

XIII.

"Cases to contain long binocular tubes should be made of strong solid wood and the tubes within so firmly made that they cannot bend; also that the glasses may at any time be taken out and the tubes differently placed, so as to be adapted to one vision or another. The upper part of the case must be so fitted with clasps that it can be closed or opened at pleasure. Long tubes of this kind can be made square like oblong beams, and may be made of plates of alloy of silver, lead, and iron, joined in several pieces, which can be easily separated from each other again. I saw a tube of this kind constructed by P. Rheita. It was at the Castle of Herbipolensus."

XIV.

"The longer the binocular the better, but the less convenient. How the inconvenience may be remedied I will show. Where these tubes are to be used they may be placed on long poles and easily extended or contracted. Tubes of this kind are very useful in war for viewing the enemy from afar, etc. I have selected the construction of a tube of this kind which I have heard P. Rheita used in his wonderful binocular telescope, whose lenses were not fitted to tubes but to a certain capsule which could be folded like a pair of bellows. The length of this was about ten hands when extended on a pole. They say that it made the moon of enormous size, which can easily be believed. The manner of constructing this kind of binocular tube is as follows: In the first place, make tubes out of leather folded like paper lanterns, or like the leather pipes used by hunters and bird-catchers for alluring beasts and birds. Crumpled leather tubes of this kind can be fitted to transverse plates, in which are placed the glasses, as in A B C D E F, Fig. 8. These plates vary in number according to the length of the tube and hold the tubes perfectly straight, and also (since they have holds within) transmit to the eye a clear image, as is the case commonly in other telescopes. The transverse tablet is shown in G H in the figure. Again, that access may be had to the ocular glasses, some part of the transverse plate B and C can protrude on either side (as is seen in J K), to which is attached the leather tubes after the manner of a capsule. When the tubes are to be attached to the pole for use, cords or little chains, *a b c d e*, are put through all the transverse tablets, and at each of these tablets a knot is made, or in some other way the exact length of the desired extension is kept. Then nails at *a* and *f* fasten the tube to the pole and hold it at the desired extension. It can be constructed as in M and enclosed in the capsule N (for it must not be kept stretched on the pole all the time), and thus conveniently carried about. Instead of a pole, a contrivance like that shown in V P can be used, which can be extended to any length and again folded together, as in figure 2, and easily carried about; also one large leather tube holding both telescopes can be made, which can be extended at pleasure like a pair of bellows. Many of the contrivances might be mentioned for showing the object right side up in astronomical binocular tubes," &c.

Binocular Microscope.

Take for the objective glass a lens equally convex on all sides, of a diameter 80–100. The principal focus will be at a distance 40–100. Now remove the object a little beyond this distance, so that the rays behind the objective lens can converge to form an image. This image will be much larger than the object (as we have already shown in formula 2), for at double the distance it will be equal to the object itself, and from double the distance till you reach the focus it (the image) will always be larger, till at the focus there will be no image at all, because there the rays behind the lens are parallel. The object placed beyond double the distance of the focus always makes the image less and less than the object, till it is placed at so great a distance that rays falling upon the lens are considered parallel and the image is found in the distance of the principal focus. All this has been already shown elsewhere. Behind this greater image two other ocular lenses are placed. One near the image itself is exceediagly convex on all sides, of a diameter 25–100. This arrangement shows the object very large and a little farther away than in ordinary composite microscopes shown in cap. 2.

On account of lengthening the tube or the greater distance of the lenses from each other (if a similar arrangement be made for both eyes), both eyes can easily look at one and the same object. The two "*prios*" lenses, ocular lenses, should be as far apart as is the distance between the two eyes, and all else arranged as in binocular tubes already explained. Such a microscope in its external form may be like the one shown in the figure, where A B is the case. Within are the two tubes, arranged as we have already shown in treating of binoculars. The object to be looked at is placed at C.

A COLLEGE MICROSCOPE.

WILLIAM H. SEAMAN, M. D.

It may be remembered that in March, 1888, "*Science*" published an article by me, maintaining the excellence of American microscopes. The train of thought inspired by that article led me to make working drawings of an instrument with some novel features. These were shown to a few friends at Columbus, and were unfortunately lost from my coat pocket at Buffalo. I did not have time to reproduce them till recently, and hoped to have the instrument itself here, but it is not quite done.

The figure shows the features which are essential, in my judgment, to a good college microscope. It will also be well adapted to the average professional man and amateur. A tripod base, rather thin, single foot back, wide open in front. The pillar may be single or double, but must have thumbscrew at the joint to hold it firm at any desired inclination; the mirror on swinging arm, adapted to carry a condenser if desired, and the stage just high enough to admit a short Abbe condenser; the center of rotation of the mirror bar just above the stage. The arm is a box-arm, Jackson model, shown with one side removed. The barrel should be of the short type, and is supported on an X-shaped bar, that slides between the V's on each inside of the box-arm, as shown by detail section. A steel tape or picture cord is fastened at each end of the X-bar, one end being the tightening screw F. This tape is wound once round the grooved wheel A, which is turned by the usual milled head and gives the coarse adjustment to the instrument. On each side of the wheel A and on the same axis are two disks, B B, that pinch the wheel A between them by a screw and act as a friction clutch. These disks are prolonged downward in the curved bar against which presses the spring E. The micrometer screw D forces the bar against this spring, and, turning the wheel A by friction, forms the fine adjustment. Every part of the instrument is adjustable for wear. The stage is a ring, with a plate of glass dropped in it. A Zentmayer sliding holder may be used. The condenser is not shown in detail, as no special features are claimed for it. I am aware that friction fittings are not new; one was described by Mr. Wenham, vol. VII,

Q. J. M.; also a chain movement was made by Pike or Grunow, of New York city, about 1850. Nevertheless, these devices do not appear to me to be as useful as that here described. The steel tape has proved successful in mechanical combinations where racks, etc. have failed, and it may succeed in the microscope. The micrometer screw D may be replaced by a cam.

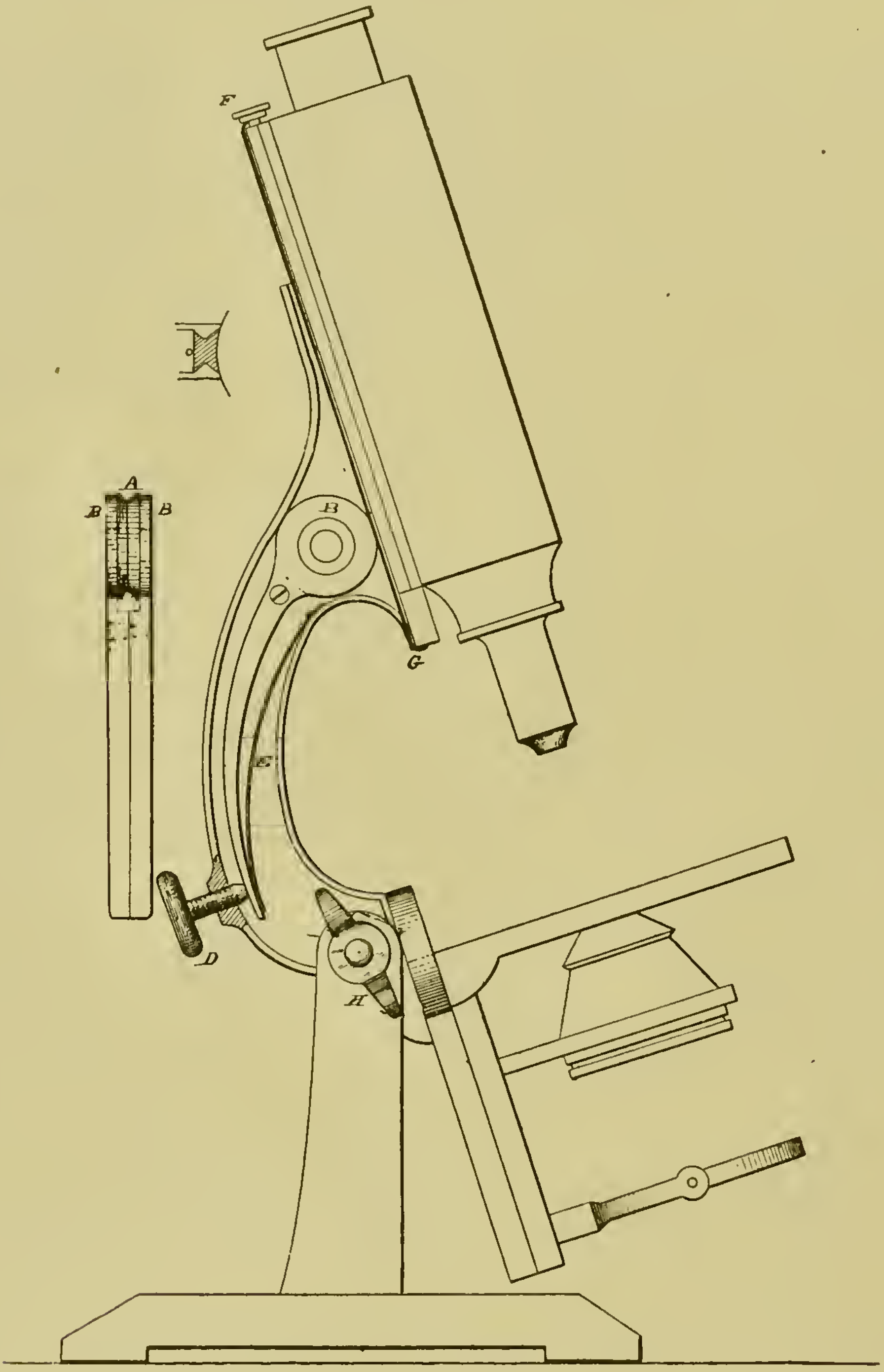


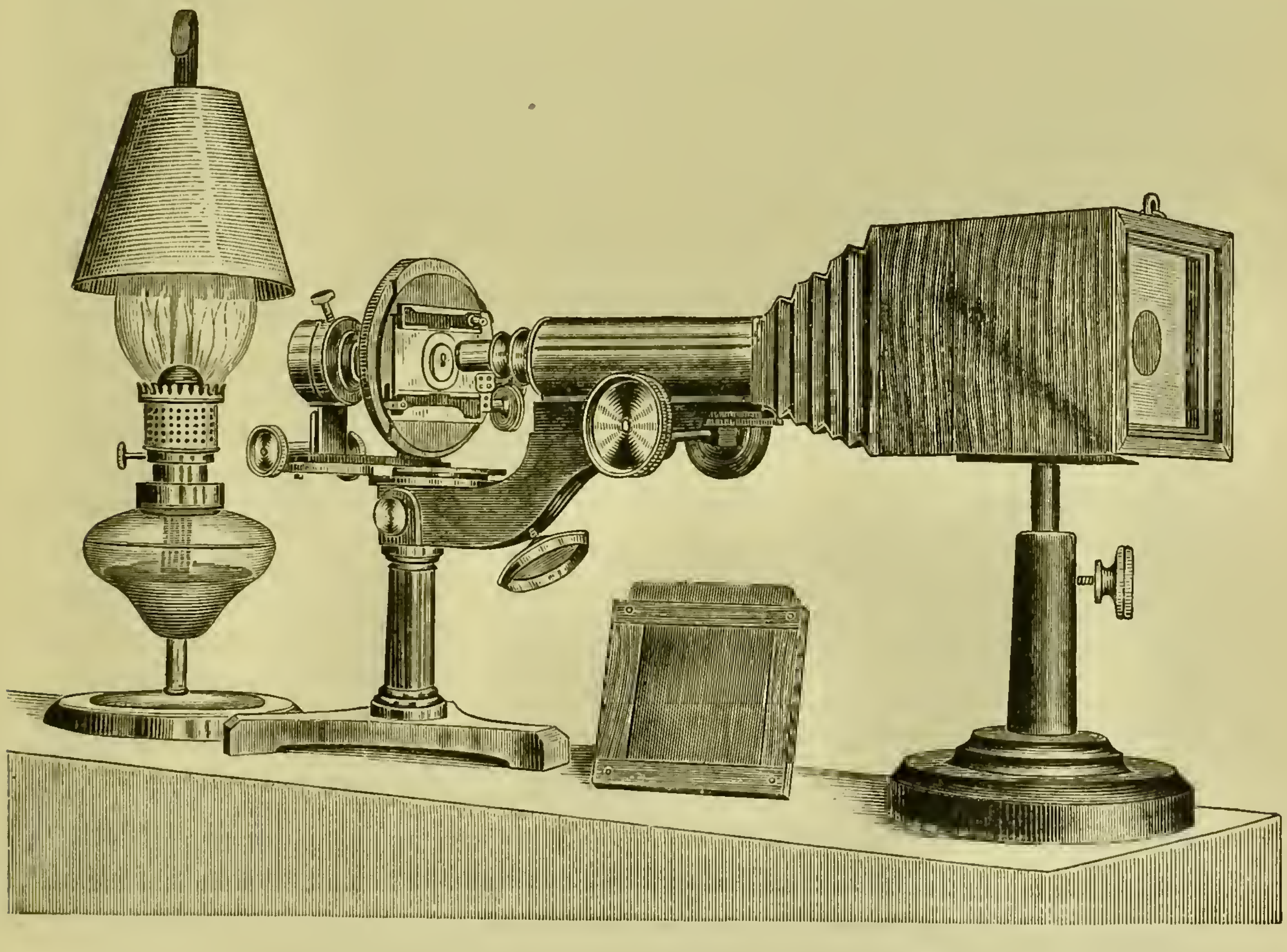
FIG. 1.

A microscope built on the plan here outlined need not be expensive, and would be capable of all but the highest class of angular work. It may be conveniently used in its simplest form, and is at the same time adapted for the successive addition of those accessories essential to the prosecution of advanced researches with the instrument. Should it prove to answer my expectations I may refer to it again.

A HANDY PHOTOMICROGRAPHIC CAMERA.

W. H. WALMSLEY, F. R. M. S.

The growing usefulness and popularity of photomicrography, by which the published work of the observer is fully and accurately illustrated, makes it desirable to have a small, simple camera that can at once be readily applied to the microscope without alteration, save bringing the body to a horizontal position. That simple means will do good work is proved by a print I have of *Pleurosigma angulatum*, X 2,400, by Spencer's 1–10 homogeneous objective, illuminated by a single-wick coal-oil lamp. It was made by Dr. J. E. Baker, of Wyoming, Ohio, and is fully equal to the best work of Zeiss apochromatics with all their appliances.



The "Handy" camera as applied to the microscope.

My friend, Mr. H. Wingate, of Philadelphia, has long been an ardent worker with the microscope, his studies being almost exclusively confined to the minute fungi belonging to the family of myxogastres. He is exceedingly skillful with the pencil, and his drawings of these minute organisms, their spores, etc., are at least equal to any that have ever come under my observation ; but, being actively engaged in business, the time wasted in making these drawings was a large tax, and he determined upon calling in the aid of photography ; and, there being absolutely no camera in the market to meet his requirements, he proceeded to construct one. Procuring a plate-holder of the proper size, he built the camera to suit it, after the plan of the man who carried the bunghole to a cooper shop to have a barrel made for it. His materials were some heavy, blackened card-board and an old piece of a steam-fitting some four inches long ; his tools a pocket-knife and a glue pot, with the brains to use them. With these crude appliances he produced a camera adapted to his microscope and capable of doing the highest class of work. He uses a Zeiss 1–18th homogeneous lens constantly, and frequently makes a dozen or more negatives of an evening therewith.

Upon seeing this little affair I was at once struck with the conviction that if it could be produced in a form adaptable to any microscope it would fully meet the long-felt want of just such an instrument. The result was the construction of the "Handy" camera, which has already been supplied to many institutions of learning and to private workers.

The camera consists of a mahogany box about 2⅝″ square, corrugated and blackened on the inside to prevent any reflections of light. A solid cone of some four inches in length, tapering to receive the tube of the microscope, is attached to the front of the box. Preferably this cone front should be in bellows form, as in the sample sent; but, being rather more costly than the solid cone, many will be satisfied with the latter. In the one case the bellows respond readily to the movements of the microscope tube in focusing ; in the other the tube must slide readily into and out of the solid cone. At the opposite end of the box is a groove, in which the plate-holder and frame containing the focusing screen slide. The former carries two plates, 2½″ square, amply large for all ordinary illustrations. Should larger-sized pictures be required they can be made by enlarging upon bromide paper. The focusing screen is made of very thin crystal glass, most carefully ground by hand, presenting the smoothest surface obtainable by this means, but still quite too coarse for the exact focusing of delicately marked objects. In fact, the focusing screen is mainly useful in procuring even and full illuminations of the field, and in properly centering the object. The final fixing of the exact focus is done by means of a focusing glass used in conjunction with a disk of thin cover glass attached to the ground surface of the screen by means of Canada balsam.

The camera is mounted upon a stout metal rod, which slides into the upright shaft of a very heavy japanned base, and can be secured at any height to suit that of the microscope (when the latter is inclined to a horizontal position) by means of a milled head. The base is shod with thick felt cloth, so that it may be placed upon any polished table-top without scratching the latter, and at the same time remain firmly fixed in the position it may be placed in.

And this is all there is of it—simple, compact, always ready for immediate service, and occupying no appreciable space upon the work-table. Although primarily intended for use with the microscope body inclined to a horizontal position, it may be as readily adapted to the latter in a vertical one, when the character of the object (as those mounted in fluids) may require. My own method has been to remove the camera from its base and mount it upon the top of an open box containing the microscope. An opening in the top of the box allows the cone to be slipped over the tube of the microscope, and in this manner I have made very successful negatives of blood corpuscles in rouleaux in their own serum, yeast spores in fluid, &c. A correspondent in Boston writes me that he has mounted the camera upon a firm retort stand for the same purpose. Many methods of using the instrument in an upright position will doubtless present themselves to the worker therewith.

The illumination may be effected by reflections from the mirror, as in ordinary work, or by removing the latter and placing the lamp behind the stage and in a direct line with the optic axis. It must be carefully centered in order to illuminate the field alike in all portions. Condensers of various kinds, bull's-eye, achromatic, Abbé, &c., can be used as desired ; but with moderate and low powers the best results will be obtained by the employment of simple diaphragms of various sizes to suit, and so placed as to come close as possible to the under surface of the slide upon which the object is mounted. All extraneous light should be excluded, so far as possible, and none be allowed to enter the objective other than the rays which illuminate the specimen. Opaque objects may be photographed quite as successfully as transparent ones, but the time of exposure will be very greatly shortened by employing direct sunlight as the illuminant, if possible.

The eye-piece may be removed or not, as the observer may elect. Following after the teachings and practice of the late Dr. J. J. Woodward, I have almost invariably worked without it, using an amplifier where sufficient magnification could not be obtained with the objective alone. In using medium and high powers I have not found the eye-piece objectionable, but with low powers it certainly detracts from sharpness of definition; so that my preference is decidedly in favor of the amplifier where an increase of power beyond that obtainable with the unaided objective becomes necessary. If possible, however, always use the latter alone. The short-tube length, alone possible (when using the "Handy" camera), renders the employment of amplifier or ocular necessary, if enlargements beyond three or four hundred diameters are to be made, since the limit of a 1–18th used direct is less than X 350.

The corrections of most modern objectives, as to visual and actinic foci, are so nearly identical that no difficulty will be experienced in obtaining sharp definition of any subject if a little care be used; but it may not be amiss to say the students' series of Bausch & Lomb are the best by all odds of any I have ever seen or used at all approaching them in moderation of cost. I have numerous remarkable examples of their work which I have never seen excelled by lenses of equal powers, no matter what their cost. It certainly is not necessary to go abroad in these latter days to get the best in the optical as well as in many other directions.

The dry plates for the "Handy" camera are furnished by the makers in two degrees of sensitiveness, to suit every variety of subject. They are readily developed by any of the methods used for gelatine plates, my own preference being given to hydrochinon, or a mixture of that with eikonogen, as giving the clearest results, clearest details, and sharpest contrasts with any desired amount of density. Their cost is but twenty-five cents per dozen—certainly cheap enough to tempt any one to their use.

It may not be amiss, in conclusion, to say a few words upon various printing methods. Presuming that every microscopist who ventures into the realms of photography will do his own printing, perhaps a few hints culled from the fields of experience may prove useful. There can be no doubt of the beauty and perfection of a good, properly toned, and finished print upon albumenized paper. This is conceded; but comparatively few amatures will ever succeed perfectly in the operations of sensitizing the paper and toning the print, while most of the "ready-sensitized" paper on the market is an abomination and a snare. Therefore discard this method of printing unless prepared to do first-class work.

Passing by platinum, as being both expensive and uncertain, excepting in the hands of an expert (although its beauty and perfection cannot be too highly extolled), let us consider for a moment the decided claim of bromide paper as being the best material for printing in our class of work. Using the smooth-surface paper and developing with ferrous oxalate, we get a perfect print, rendering the most delicate details with the crispness and clearness of a steel-plate engraving, which, indeed, it most closely resembles in very many instances. The exposure is made by lamplight, so that one is entirely independent of time or weather, and the finished print is absolutely permanent, as much so, it is reasonable to believe, as a carbon print. If the sheet be allowed to dry spontaneously it will present the appearance of an ordinary plate engraving. If a polished surface be desired, all that is necessary will be to float the paper, print side down, upon a sheet of polished hard rubber, to squeeze it into optical contact, removing all superfluous moisture, and when quite dry it will peel off the rubber plate with a beautiful polished surface, greatly increasing the delicacy of detail in many subjects, especially diatoms. Most decidedly my preference in given to this form of printing.

But there is another method which, at the risk of being laughed at, I am inclined to gently urge upon my brethern of the tube. I refer to the ferro-prussiate, or more commonly named "blue prints." This method of printing is tabooed in many instances, "blue prints" being vigorously proscribed in the albums of The Postal Photographic Club; but for all that it has decided advantages and merits for the work we are considering. It is cheap, as the paper may be purchased ready sensitized at a very trifling cost, and it requires no skill or experience in the using. It is merely necessary to expose to bright sunlight until sufficiently printed (a few experiments will determine this), and then to wash in several changes of water, the result being a bright, permanent blue print upon a clear white ground, with excellent detail, excepting in the most delicate structures.

The negatives made with the "Handy" camera are of a convenient size for printing lantern slides by contact. A print on glass is certainly the most perfect of any that possibly can be made, and the importance of this method of demonstration has long since been conceded. Gelatine plates, coated on thin glass with special slow emulsions, are furnished by several makers, and any microscopist can readily make his own lantern slides with a little expenditure of time and patience. I submit with this paper specimens of printing in all their various methods for "Handy" camera negatives; also a reproduction by one of the many block processes now in use for book illustration. True, photogravure is the best process for such work, but in many cases the cheaper one herewith shown answers an excellent purpose. One of the cameras is also sent for the inspection of members interested in the subject, as I trust the majority of them will be.

A COVER-GLASS SUPPORT FOR SOLID MOUNTS.

Howard N. Lyon, M. D., Demonstrator of Microscopy in the Hahnemann Medical College of Chicago and the Chicago College of Dental Surgery.

For a number of years I have been conducting a series of experiments with cover-glass supports other than cement cells. The desiderata being permanency of the mount and rapidity of its preparation, cement cells will not answer my purpose on account of the time necessary to prepare them properly and the large number that I would be obliged to keep constantly on hand, and tinfoil, glass, etc., have not been satisfactory to me.

The method I have had in constant use for over a year, with perfectly satisfactory results, consists in a ring made as follows: A piece of silver suture wire of suitable thickness is bent around a mandril and the ends cut off where they meet. The ring is now placed on a flat metal block, as the bottom of a flat-iron, and flattened by a few blows of a hammer. This will spread the ends of the ring slightly.

In use it is placed on the slide and filled with moderately thick balsam. The specimen is then placed on top, and after a few moments the cover put on and a clip applied. Bubbles pass out with the surplus balsam expelled on the side of the opening, and the balsam should be left on the slide for a few days, to go back into the cell and supply the deficiency caused by the evaporation of the solvent.

When hard, the slide is cleaned and a preparatory coating given of shellac, followed by a ring of Brunswick black.

The mandril that I use is made of maple, and consists of a series of steps, each size producing a ring slightly smaller than the corresponding cover.

Very thick specimens are soaked for twenty-four hours, or longer if necessary, in very dilute balsam before being transferred to the cell.

TWO NEW FORMS OF STAGE MICROMETERS.

Dr. M. D. Ewell, 97 Clark Street, Chicago, Ill.

As already stated in another paper read at this session, the writer prefers a stage micrometer ruled upon a metal surface to any other form, and never uses a glass stage micrometer. Inasmuch, however, as it is somewhat more convenient in ordinary work to use transmitted light, glass stage micrometers are almost universally employed. In the use of glass there are several difficulties: First, the unstable nature of the material and the danger of deterioration of the lines by use or otherwise, unless they are filled and protected by balsam and a cover glass in the ordinary manner of making balsam mounts. When filled with graphite and thus covered, the lines are durable; but ordinarily a filled line has not the clear, sharp outline of a good unfilled line; so that this method has its disadvantages. Micrometers with the lines ruled either on the glass slide or under side of the cover glass and mounted dry are very liable to "sweat." Various devices have been resorted to to prevent this; among others, that devised by Professor Rogers, of mounting the cover glass on the top of a metallic ring perforated for the purpose of allowing ventilation. While this usually accomplishes the purpose for which it was designed, micrometers thus mounted are in my experience very fragile, and not infrequently the ring becomes detached from its base. Very likely this can be prevented by the use of greater care by the possessor of such a scale.

In order to arrive at the desired result, the following device is proposed:

Figure 3 represents a view of the micrometer from above. "*a*" is a slide, 3″ x 1″, made of polished brass, either nickel-plated or oxidized, with a circular opening, with two or more wings "*d*" extending through the plate, as represented in the figure; "*b*" represents a cover glass, on the under side of which the scale is ruled.

Figure 4 represents a sectional view of the slide, with the cover glasses "*b*" and "*c*" resting on and cemented to shelves turned out of the periphery of the circular opening on each side of the slide. It will be observed that there is a clear space between the two cover glasses, and the wings or openings (*"d" "d,"* Fig. 3) may be made numerous enough to secure perfect ventilation. The lower cover glass need not be permanently cemented in place, but only temporarily fastened by bits of wax, so that it may be removed whenever necessary for the purpose of brushing off the dust from the under surface of the ruled cover in case it becomes necessary to do so. One advantage claimed for this form of mounting is that the ruled cover, by reason of its lying in a recess below the surface or, at most, flush with the surface of the slide, is not so likely to be displaced or broken by accidental violence. Another advantage is that the ventilation can be made perfect.

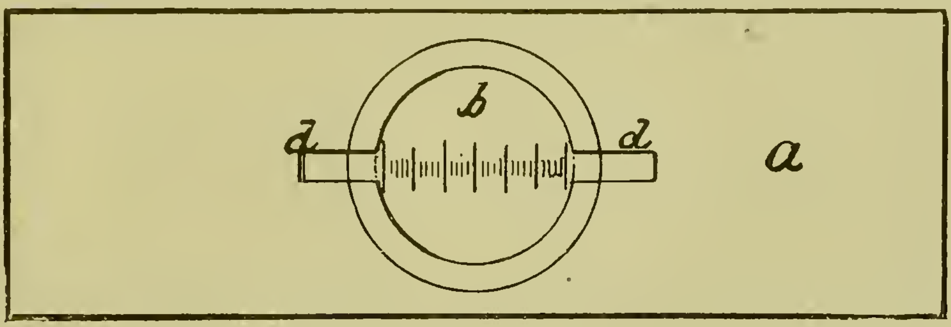


FIG. 3.

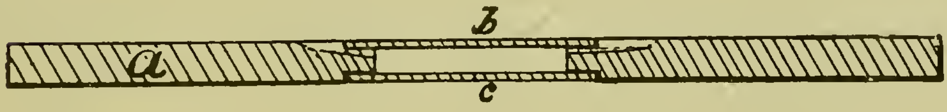


FIG. 4.

Another form of micrometer is the following: A plate-glass slide is first coated with a very thin film of pure silver. The one shown the Society was prepared for me by my esteemed friend, Mr. John A. Brashear, of Allegheny City, to whom I desire to express my obligation for many courtesies extended when in need of assistance to "help me out of some scientific corner."

The film is precisely the same as he uses on his excellent glass reflecting telescopes. It will be observed that it is so thin as to be translucent. Lines are ruled *through* this film of silver precisely as if it were a glass slide. The lines and film of silver are then protected by balsam and a cover glass in the ordinary manner. This form of micrometer possesses the advantages of both glass and metal micrometers. It can be used either with transmitted light or with an opaque illuminator and, with reasonable care, is indestructible. While the lines on an ordinary glass stage micrometer *can* be seen with an opaque illuminator, it is not easy to use one by reflected light, and their continual use is not recommended as conducive either to pleasure or piety. Lines ruled on metal, on the other hand, are by reflected light so clear and pleasant to use that, once familiar with them, no other form is desirable. I refer now only to the use of reflected light, which is usually used in making comparisons of lengths that are greater than the width of the field of the microscope. Of course, if the spaces of the scale to be used can be included within half of the field of the microscope (no more than which should ever be used in micrometry, and that only in the center of the field), transmitted light, as already stated, is more convenient and is employed by most workers.

In presenting this scale to the Society as a new form of micrometer I may state that I am acquainted with the use of the test plate manufactured by Zeiss, in which the lines are ruled through a thin film of metal; but I am not aware that such a device has ever before been used for micrometric purposes, and in the plate of Zeiss in my possession the lines are so ragged and the film so opaque as certainly to be unsuited for any such purpose.

THE EFFECT OF CURVATURE OF THE COVER-GLASS UPON MICROMETRY.

Dr. M. D. EWELL, 97 Clark Street, Chicago, Ill.

Inasmuch as by far the greater number of observations with the microscope are made upon objects under a cover-glass, it becomes an important subject of inquiry whether microscopic measurements are sensibly affected by the curvature of the covers. Especially is this true as to such experts as profess to be able to identify blood by the measurement of the red corpuscles. I have not been able to find in the literature any investigation of this subject, and having in the Cronin case been called as an expert with reference to the identification of blood by micrometric measurement of the red corpuscles, I made the observations herein recorded not with a view to publication but in order to be able to come to a correct conclusion as to one factor in that case.

In the comparison of long standards this question has no importance whatever, inasmuch as they are always uncovered; but inasmuch as most objectives in common use are corrected for covered objects or are usually so used, the value of one division of the micrometer used with such objectives on covered objects must be determined from a micrometer having its lines covered either permanently or temporarily.

It is obvious that sensible curvature of a cover-glass interposed between the object and front of the objective must affect the amplification of the objective. If such curvature is spherical this effect will be symmetrical. If the curvature is irregular it must distort the object, so that the interval between two objects will appear to be different according to the orientation of the cover-glass.

Several methods are available for testing the curvature. It may be tested by viewing with a telescope the image of some regular object, such as a building reflected from the cover-glass. It may also be tested by accurately focusing a telescope upon some object, such as an artificial star, and then interposing the cover-glass to be tested between the object viewed and the objective of the telescope. If the cover is flat and if its sides are parallel, such interposition will have no effect upon the image other than very slightly to diminish the amount of light received from it. If the cover-glass is spherical it will change the focus of the telescope, which must be refocused in order to obtain a clear image of the object observed. If the cover-glass has irregular curves the image will also be distorted. If the cover-glass is prismatic the image will be displaced laterally.

A very much more sensitive method than either of the above is that by the observation of the Newton's rings caused by placing the cover-glass upon a flat surface. For this purpose I obtained one of Mr. Brashear's justly celebrated flat test sufaces; but having found upon trial that the second method above described was sufficiently sensitive for my purpose, it was used exclusively in the observations hereinafter described. Tested by this method, I may state that nearly all the cover-glasses in common use possess either regular or irregular curvature. Tested by the method of Newton's rings, I am informed by Mr. Brashear that it is practically impossible to find one perfectly flat. I have myself examined and tested a large number furnished me by the courtesy of the Bausch & Lomb Optical Company and The Palmer Slide Company, as well as some purchased of Mr. Zeiss of Jena, and have found a very few that by the second method above described were sensibly flat. By far the greater number possessed quite sensible curvature. My method of determining whether the curvature of the cover-glass sensibly affected micrometric measurements was carefully to measure the same space under identical conditions, except as to the cover-glass, and to compare the results. For this purpose a high-angled dry objective was employed as being more likely to be affected by a variation in the cover-glass than a homogeneous immersion or low-angled objective. The cover-correction was made as perfect as possible and not changed thereafter.

The measurements were all made with a Bausch & Lomb first-class ⅙ objective (cover-correction $= 0 + 3 + \frac{1}{2} + \frac{2}{10}$), a Bullock filar micrometer, a Smith's vertical illuminator, manufactured by Mr. W. H. Bullock, and a Bullock stand, "Professional No. 2."

The space measured was the second 1–100 mm. of "Centimeter A," counting from the first line of the 1–100 mm. band. The temporary covers, except as otherwise noted, were 16–100 mm. in thickness, and all except Nos. 1, 7, and 9 were taken at random from covers purchased in the ordinary course of trade or furnished by the courtesy of the Bausch & Lomb Optical Company and the Palmer Slide Company. No. 1 was so nearly flat that its curvature, if any, could not be detected by the second telescopic test above described. Nos. 7 and 9 were covers ground and polished to order by Messrs. Bausch & Lomb with the express purpose of making them as nearly flat as possible, but with the effect of making them more curved than covers usually are which have not been thus treated, experience showing that the covers are so easily warped that they cannot by ordinary methods be ground flat.

The measurements are recorded in terms of divisions of the micrometer, of which 1 division = 0.0732 μ. The number at the head of each series refers to the temporary cover used in that series.

Oct. 6, 1889.

No. 1 (flat cover).	No. 2.
131.4 div.	132.4 div.
131.2 “	132.5 “
133.0 “	131.6 “
132.0 “	130.5 “
133.0 “	131.9 “
Mean: 132.1 “	Mean: 131.8 “

No. 3.	No. 4.
130.0 div.	131.7 div.
3.3 “	3.9 “
1.9 “	1.0 “
0.7 “	2.9 “
1.8 “	1.0 “
Mean: 131.5 “	Mean: 132.1 “

No. 5.	No. 6.
131.9 div.	131.7 div.
1.0 “	1.7 “
1.8 “	2.5 “
1.2 “	2.4 “
1.7 “	1.1 “
Mean: 131.5 “	Mean: 131.9 “

No. 7 (B. & L., ground and polished).

131.9 div.
2.8 "
4.0 "
2.9 "
1.0 "

Mean: 132.5 "

No. 8 (Zeiss).

131.4 div.
1.6 "
0.6 "
1.1 "
1.1 "

Mean: 131.2 "

No. 9 (B. & L., ground and polished).

132.2 div.
3.4 "
2.2 "
2.4 "
1.2 "

Mean: 132.3 "

No. 10 (No. 1 measured again).
(No. 1.)

132.6 div.
3.0 "
1.5 "
4.0 "
2.5 "

Mean: 132.7 "

No. 11 (cover $\frac{8}{100}$ mm. in thickness, and hence, there being no change of the cover correction, bad definition).

132.2 div.
1.8 "
3.1 "
2.5 "
3.7 "

Mean: 132.7 "

No. 12 (No. 1 again).
(No. 1.)

133.0 div.
132.3 "
131.0 "
131.0 "
129.5 "
132.5 "

Mean: 131.6 "

	Mean.	Difference from mean.
(1) (flat cover)	132.1 div.	— 0.1 div.
(2)	131.8 "	+ 0.2 "
(3)	131.5 "	+ 0.5 "
(4)	132.1 "	— 0.1 "
(5)	131.5 "	+ 0.5 "
(6)	131.9 "	+ 0.1 "
(7) (B. & L.)	132.5 "	— 0.5 "
(8) (Zeiss)	131.2 "	+ 0.8 "
(9) (B. & L.)	132.3 "	— 0.3 "
(10) (1, second time)	132.7 "	— 0.7 "
(11) (cover $\frac{8}{100}$ mm.)	132.7 "	— 0.7 "
(12) (1, third time)	131.6 "	+ 0.4 "
Mean of all 12 series	132.0 "	

Mean of all, excepting the series with the thin cover (No. 11), = 131.9 div.

Mean of the eight series, excluding the three series of measurements with No. 1 and the series with the thin cover, = 131.9 div.

Mean of the three series of measurements with No. 1, the flat cover, = 132.1.

An inspection of the above measurements discloses the fact that the differences from the mean in the case of the three series in which the flat cover was used are as great as when other covers were used, and that the mean of all the measurements using the flat cover is sensibly the same as with the other covers (some of which possessed a curvature quite sensible), the difference being only 0.02 μ., which is too small to measure.

It was intended to make a much larger series of measurements, but the pressure of other work prevented.

The foregoing are, however, sufficient to warrant the conclusion (to which there may possibly, in the case of very bad covers, be some exceptions) that using good cover-glass, such as is furnished by reputable dealers in this country, the most, if not all, of which is manufactured by Chance, the influence of the curvature of such covers is practically *nil.*

This is not the conclusion to which the writer had come from *a priori* reasoning ; but it is the only one warranted by such facts as have been observed. Further observations will be presented in a future paper.

DESCRIPTION OF THE STANDARD CENTIMETERS MANUFACTURED IN PURSUANCE OF THE RESOLUTION OF A. S. M. ADOPTED IN 1889.

Dr. M. D. Ewell, 97 Clark Street, Chicago, Ill.

In the preparation of these five scales no attempt has been made to copy "Centimeter 'A.'" Even if the attempt had been made, it could not be known whether the scales so made were really copies without a careful comparison of them with the original, and, if their corrections are known, it matters not whether they are copies or not. They are, as will be seen, all ruled on plate-glass, three inches long by one wide. Considerable difficulty was experinced in getting suitable glass for this purpose, but the glass used will, it is believed, answer every requirement. The diamond used is one that has been in use for some time, and it is believed that the lines will prove durable, though time only will settle the question. The centimeters and their subdivisions ruled on these plates were all ruled with the same settings of the index of the screw and under as nearly the same conditions as to temperature, etc., as could be commanded. Their errors, therefore, should be very nearly the same. Each centimeter is subdivided into ten millimeters, the first millimeter into tenths, and the first tenth into hundredths. The defining lines are single, differing in this respect from "Centimeter 'A,'" in which they are triple. The last defining line of the last millimeter of each scale is the middle line of a triplet, three lines being ruled at the end of each scale to facilitate the identification of the separate millimeters. Two lines about 20 $\mu\mu$. apart are ruled at right angles to the defining lines. All measurements should be made between these lines. For purposes of identification they have been marked "1 cm., Ewell I, etc., to V." These scales were sent to Prof. W. A. Rogers on the 16th of July, 1890, for investigation and report to this Society.

The writer very much prefers a scale ruled on a metal surface, as giving a better and more durable line, and as in every other way more suitable for micrometric purposes. The fact that such a scale cannot be used with transmitted light is an objection of no weight, for by using a Smith's vertical illuminator it can be investigated with the highest powers. The writer has often used his Zeiss 1–18 on such a scale with great satisfaction.

Should the proposition meet with favor the writer will undertake to make for the Society, in ample time for thorough investigation before the next meeting, one or more standard centimeters on speculum metal surfaces by Brashear, or on silvered glass as may be preferred, which, when investigated, may serve as sub-standards, obviating the necessity of using the original "Centimeter 'A'" except on rare occasions. No charge will be made for this service beyond the expense of the surfaces, which for two such scales the writer will undertake shall not exceed $6.00. The expense would not be even so large as this, but for the fact that even with great care one is liable to spoil one or more surfaces before getting a satisfactory ruling. With these sub-standards and the glass scales herewith presented, which will serve for more common use, the micrometric standards of the Society would need no further addition to their number and would subserve all the purposes for which they are intended.

THE MICROSCOPE AND CAMERA IN THE DETECTION OF FORGERY—EXEMPLIFIED BY LANTERN SLIDES AND PHOTOGRAPHS OF SIGNATURES IN THE JEROME WILL CASE.

Dr. M. D. EWELL, 97 Clark Street, Chicago, Ill.

The subject of this paper is one of great practical importance in the administration of justice; and, while not undertaking to treat the subject exhaustively, we shall endeavor to give some points which may be of value in subsequent cases.

The modes of committing forgery are various. Forgery may be committed in the following ways :

(1) By alteration of the document in question, which may consist of (*a*) an erasure or erasures; (*b*) of additions to the instrument; (*c*) of both erasures and additions.

(2) By the forgery of the entire writing or of the signature. This may be accomplished in several methods: (*a*) By tracing a fraudulent signature over a genuine signature by means of the pen or pencil, and (*b*) by copying or imitating the genuine signature otherwise than by tracing.

The methods of detecting frauds thus committed are various, according to the nature of the fraud :

1st. Composite photography has been proposed as a means of determining the authorship of a disputed document. While this method seems to be founded on correct scientific principles, yet in our opinion the cases in which it may be applied in practice will be very few, if any. In order to apply this method for the indentification of a writing whose authenticity is questioned, very much more material is required than is usually available in any case presented in court. As a rule, questions of authenticity arise principally with reference to disputed signatures, and under the rules of evidence applicable in England and in most of the States it is very difficult, if not impossible, to procure other similar signatures as a means of identification; and without a very considerable number of similar signatures this method cannot be adopted. Moreover, the difficulties of technique are such as to render it impracticable in the hands of an ordinary observer.

2d. Another means of identifying the authorship of a document is that proposed by Prof. T. C. Mendenhall, and published, I believe, in *Science* some years since. This method consists in what may be styled "Curves of Literary Style," the co-ordinates of which, if I remember correctly, consist of the number of words and the number of syllables which they respectively contain. This method, although very interesting and probably of considerable scientific value in cases to which it is applicable, is not, in the opinion of the writer, of any practical value in the ordinary administration of justice, as cases are presented for adjudication in court, for the reason that it requires vastly more material than is ever accessible in ordinary practice.

3d. The ordinary method of identifying writing in use in courts of justice is that styled "Comparison of Hands." In this connection a brief review of the rules of law applicable to this case may not be inappropriate. By the English common law a witness is competent to testify respecting the genuineness of a disputed writing:

(1.) If he has seen the party alleged to have made the writing in question write; and it is sufficient for this purpose that the witness has seen him write but once, and then only his name.

(2.) The second mode of acquiring knowledge of the handwriting of another is by the receipt from such person of written communications purporting to be in his handwriting, either in the usual course of business or in reply to letters written by the witness, provided such communications have been acted upon as genuine by the parties, or adopted as such in the regular course of business.

(3.) Another method is by means of the comparison of the specimen in question with fairly selected undisputed specimens of the alleged handwriting. With respect to this third method there is considerable conflict of authority. By the English common law such comparison was permitted in two cases—(*a*) where the writings in question are of such antiquity that living witnesses cannot be had, and yet are not so old as to prove themselves. Here the course is to produce other documents, either admitted to be genuine or proved to have been respected, treated, and acted upon as such by the parties, and to call experts to compare them, and to testify their opinion concerning the genuineness of the instrument in question; (*b*) where other writings admitted to be genuine are already in the case.

Considerable diversity of practice at present prevails in England and in the various States of the Union; this diversity has been brought about partly by statutory enactment, and partly by decisions of the courts. Without undertaking to go into the details of the subject, we may state that in the State of Illinois the English rule is applied with some strictness, and excluding the case of ancient documents, the only case, as we understand it, in which a comparison of hands by experts is permitted is where other writings admitted or proved to be genuine are properly in evidence, and pertinent to the case. (Brobston *v.* Cahill, 64 Ill., p. 356, in which the rule laid down in Jumpertz *v.* The People, 21 Ill., 408, is explained and qualified. See also in general 1 Greenleaf on Evidence, sec. 577 *et seq.;* Chamberlayne's Best on Evidence, sec. 232 and note; Rogers on Expert Testimony, sec. 139, 140, *et seq.*)

With reference to this third method by comparison of hands, two cases arise—first, where the material upon which the judgment is based consists of the disputed and genuine signatures; and, second, where the material at hand consists of a letter or letters, or other documents more voluminous. In the former case the judgment arrived at does not, of course, possess the same weight as where more material is at hand upon which to form a judgment; nevertheless, cases do arise in which the expert is warranted upon a comparison of the signatures in expressing a very clear opinion that the signatures were not made by the same person.

As to the method of arriving at an opinion upon the comparison of one or more other signatures, the cases are so diverse that no general rules can be laid down. Each case must be decided upon its own particular facts.

In the second case, not unfrequently a conclusion can be arrived at having a high degree of probability, amounting almost to a moral certainty. In arriving at a conclusion, many things are to be considered—not only is the form of the letters important, but their manner of combination to form words is even more important. The use of capitals, punctuation, mode of dividing into paragraphs, of making erasures and interlineations, idiomatic expressions, orthography, mechanical construction, style of combination, and other evidences of habit are important elements upon which to form a judgment. An interesting case of this kind occurred in the Greenwich county court, in which the party denied most positively that a certain receipt was in his handwriting. It read: "Received the Hole of the above." Upon being asked to write a sentence containing the word "whole," he took pains to disguise his hand, but used the above phonetic style of spelling, even writing the capital "H," and then he ran away to escape prosecution for perjury. (Rogers Expert Testimony, Sec. 146. Taylor on Evidence, sec. 1669, note 1. Greenleaf on Evidence, sec. 581, note.)

Some years since two anonymous letters, together with a number of letters written by several different persons, and the minutes of a scientific meeting written by a party not suspected of being the author of the anonymous letters, were submitted to the writer for his opinion. A careful study of the documents led the writer to the conclusion that the anonymous letters were written by the writer of the minutes above referred to; this conclusion was so much at variance with the opinion of the party who submitted the documents for examination that he was disposed to reject it. The writer, however, persisted in his opinion, and upon confronting the supposed author of the anonymous letters with the opinion, and accusing him of the authorship of said anonymous letters, he broke down aud acknowledged himself to be their author. In this case while the form of the letters in the several documents was not by any means identical, yet the manner of combining the several letters to form the more common particles, such as "the," "and," "of," "to," "for," etc., was identical in every instance, thus demonstrating to the mind of the writer the identity of their authorship.

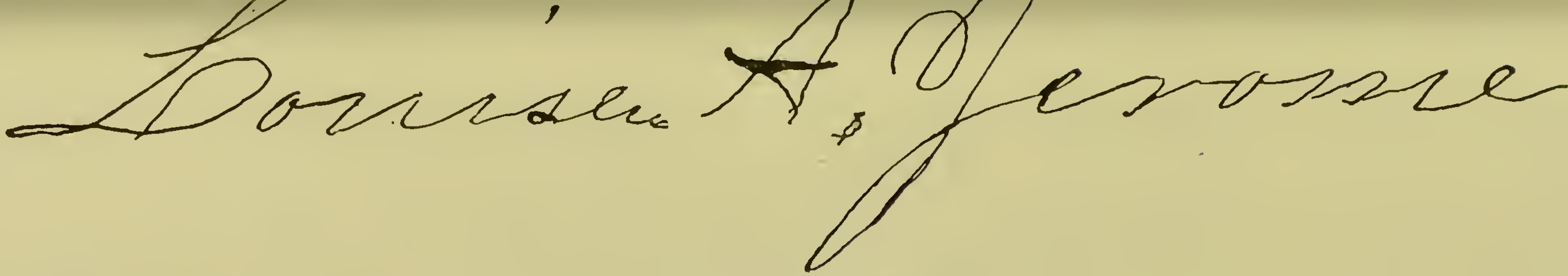
Perfect identity of two signatures is very strong, if not conclusive, evidence of fraud. No two autograph signatures by the same person will be exactly alike. In the famous Howland Will case, 4th Am. Law Review, 625, 649, (also reported in the American Law Register, Sept., 1890, p. 562,) Professor Pierce, at that time Professor of mathematics in Harvard University, testified that the odds were 2,666,000,000,000,000,000 to 1 that an individual could not with a pen write his name three times so exactly alike as were the three alleged signatures of Sylvia Ann Howland, the alleged testatrix of the will and two codicils. If, therefore, upon superposition against the light two signatures exactly coincide, it is morally certain that one of them is a forgery.

(4.) Another means of detecting forgery is by the internal evidences of fraud afforded by the writing itself, with or without the aid of comparison with other and genuine writings.

These internal evidences may consist of alterations, such as erasures, additions, etc., above described, or of tracings of the genuine signature by means of a pen or pencil, which tracings are afterwards inked over with a pen, or they may be found in a copy of a genuine signature otherwise than by tracing, in the several manners above described. The copy or imitation of the genuine signature may be either free-hand or composite; by which latter is meant that the signature is made discontinuously, or by piecemeal. The detection of frauds attempted in the manner first above described is comparatively easy. A very low power of the microscope will readily reveal the erasures, and, not unfrequently, the word erased may be made out. When the signature has been traced over a genuine signature, usually the forger will be found to have failed to entirely cover the original tracings, the character of which, by the aid of a low power, can usually be satisfactorily made out. In this case, also, the signature will usually be found to be discontinuous, and the places where the pen has been put upon and removed from the paper in endeavoring to cover up the original tracings can be readily made out, and when thus made out this fact is strong, if not conclusive, evidence of fraud. When the signature has not been traced, but is composite or made by piecemeal in the manner above described, this can almost always be satisfactorily made out by the use of a low power, and when the composite character is so made out it is likewise strong, if not conclusive, evidence of fraud. Not unfrequently by the aid of the microscope it can be determined that alterations of the instrument were made with a different pen and with different ink; and not unfrequently the order in point of time in which they were made can likewise be determined. In questions of this sort, and in general in cases of disputed signatures, photography is of very great service. In the comparison of disputed signatures the writing in question should, if possible, be compared with the original and not with a photographic copy, such copy being considered by most courts as secondary evidence; nevertheless, photographic enlargements of genuine and disputed signatures, the correctness of which is established by testimony, are very useful as a means of illustrating the evidence of the expert. Not unfrequently, also, by the aid of photography differences in ink may be made out which are insensible to ordinary observation.

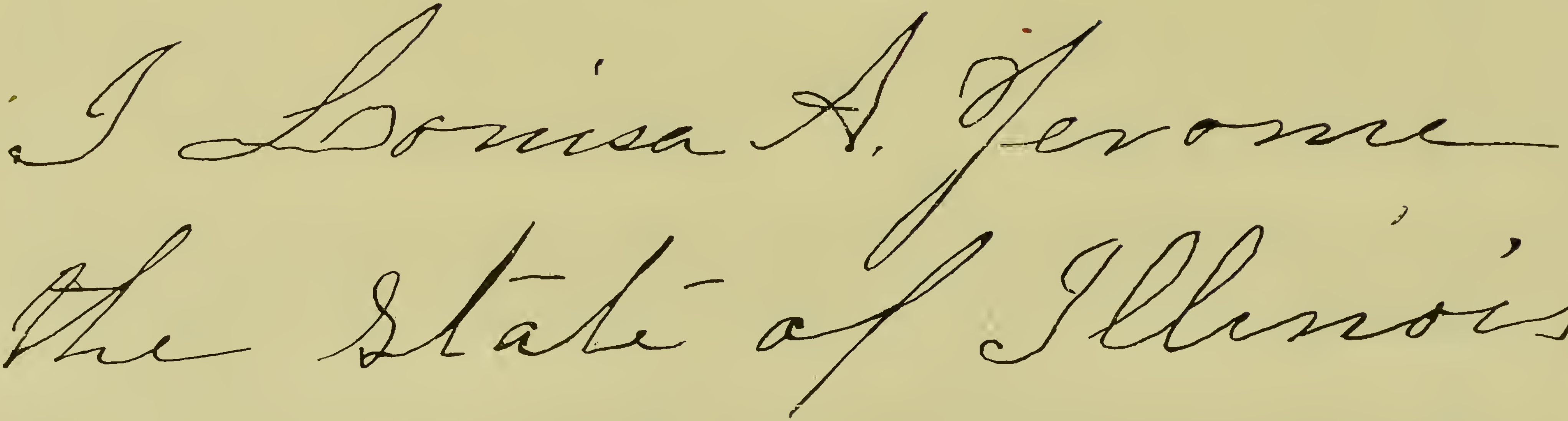
Many of the points above discussed were well exemplified in the Jerome will case, recently decided by the Probate Court of Cook county. In this case a most audacious attempt was made to impose upon the court a forged for a genuine will. The case turned upon the authenticity of the signature to the will, there being no dispute as to the handwriting in the body of the will. The signature in question (a bromide enlargement of which, together with lantern slides, are herewith presented for consideration) was what I have above styled as a composite signature. At what I may style the cardinal points of the signature there appeared upon the paper in immediate juxtaposition to the signature numerous indentations, which appeared to have been made with an instrument with a somewhat rounded point; these indentations, with two or three exceptions, did not come in contact with the writing. The writer was clearly of the opinion, and so testified, that in his opinion, from their position, they were made as caliper-marks or guides to the signature subsequently written. In one instance, namely, at the top of the initial " L," the mark took the form of a line extending across the loop of the letter " L," and the writer was able, by microscopic examination, to testify to the opinion that it was made before the signature was written; as also was the case with respect to one or two other of these indentations. The signature was likewise a composite one, from the fact that the pen of the writer appeared to have been removed from the paper at unusual places, forming breaks in the continuity of letters which are usually made by a continuous motion of the pen. This is well exemplified by the photographs and by the lantern slides. An examination under a low power likewise revealed the fact that in a number of instances the signature had been patched, or the lines retraced in certain portions where such patching or retracing would not ordinarily occur if the signature were a natural or genuine signature.

From the combination of these three different classes of facts the writer was able to testify to the opinion that the signature was a forgery. At this time there was not in the case any genuine signature with which the signature in question could be compared, and under the laws of the State of Illinois, as I have already stated, no signature could be used for such purposes unless properly in evidence in the case. This want the proponent's counsel supplied by his cross-examination of one or more witnesses put upon the stand by the party opposed to the probate of the will. Enlarged copies of two of the signatures thus received in evidence and lantern slides thereof are herewith presented for your consideration. The differences between the forged and the genuine signatures will be apparent from the examination of these enlargements. Without detaining you longer for the purpose of considering these specific differences, I may state that the court was of the opinion that the signature was not a genuine signature, and probate of the will was refused. The party alleged to have committed the forgery is now under indictment.



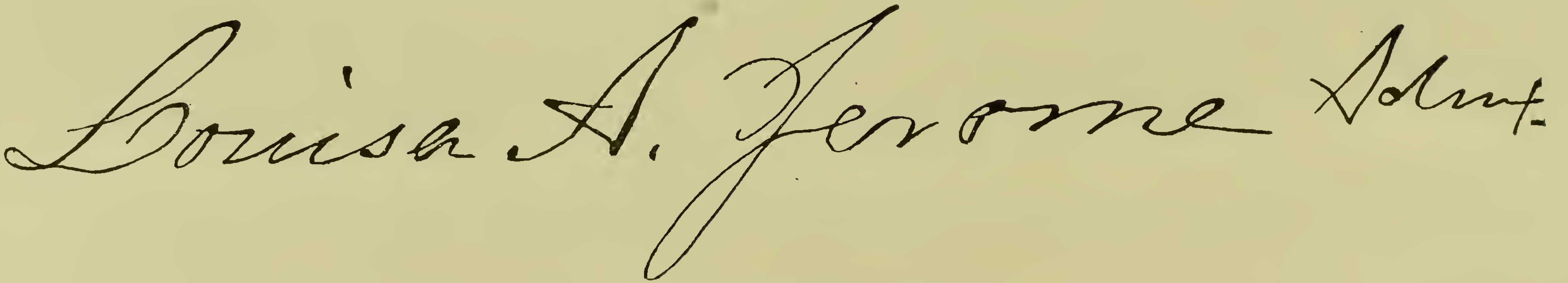
Louisa A. Jerome

FIG. I.—The Forged Signature.



I Louisa A. Jerome
the State of Illinois

FIG. II.—Genuine Signature to an unexecuted will.



Louisa A. Jerome Admx

FIG. III.—Genuine Signature to a Check.

RHIZOPODS OF OAKLAND CO., MICH.

Stuart H. Perry, Pontiac, Mich.

The list of species which I present in this paper is the result of two or three years' desultory observations, and, of course, cannot be considered an exhaustive one, of the Rhizopods of Oakland county. The localities I have examined comprise chiefly the numerous lakes, ponds, and streams in the vicinity of Pontiac, and sphagnum and algæ growing in swamps and other cool and shaded localities. As a rule, I have found Rhizopods most abundant in sphagnum from well-shaded situations, although the largest number and greatest variety of Rhizopods I ever observed in a single gathering was from Clinton river, among the fine thread-like roots of the willow. I find their habits substantially as described by Professor Leidy in his monograph on the Rhizopods of the United States, which has been my chief authority in classifying the species I have found. I have noted, however, that many species, including *Amœba*, are not so active and voracious as those described by Professor Leidy, it being quite rare to find one containing a diatom or a desmid, while many figured in his work are filled with colored food-balls and vegetable particles.

I have observed the following species :

Amœba proteus
“ radiosa —common.
Ouramœba vorax—occasional.
Dinamœba mirabilis—in sphag., rare.
Difflugia pyriformis
“ urciolata
“ acuminata —common.
“ globulosa
“ constricta
“ corona
“ lobostoma —occasional.
“ spiralis—abundant in a single gathering (sphagnum).

Arcella vulgaris } —very common.
" discoides }
" dentata—occasional.
" mitrata—rare.
Euglypha alveolata } —common in sphagnum.
" ciliata }
" cristata—rare.
Cyphoderia ampulla—common in one or two localities.
Campascus cornutus—a single characteristic specimen observed. Described by Leidy in Rhizopods of the United States and found by him only in China Lake, Uinta Mountains, Wyoming.
Centropyxis aculeata—very common.
Trinema enchelys—common in sphag.
Clathrulina elegans—not uncommon.
Vampyrella lateritia—very abundant in a bottle w. œdogonium in my laboratory, rare elsewhere.
Hyalosphenia elegans—rare in sphagnum.
Actinosphærium eichhornii—common.
Actinophrys sol—very common.
Pseudodifflugia gracilis—in sphag., rare.
Acanthocystis }
Radiophrys } —occasional.
Heterophrys }

Rhizopods are, of course, best studied alive; but the testaceous forms, and even the soft, unprotected varieties, may be mounted so as to display very well many of the characteristics of the species. As the specimens are rarely sufficiently abundant to warrant the treatment by levigation, that so facilitates the separation of the foraminifera and diatoms, it is better to pick them out singly with a small camel's-hair brush under a dissecting microscope and transfer them to a drop of glycerine, where they may remain without change until required for mounting. Occasionally, when a certain variety is very abundant, the isolation of the forms may be facilitated by carefully washing the material and finally transferring the specimens singly, each variety to its own slide. For most species I have found glycerine jelly to be the most suitable medium for mounting, as the specimens do not become too transparent, do not change their position on the slide as in a fluid, and may be transferred directly from the glycerine. The sarcode also is preserved, though of course somewhat contracted and with the pseudopods withdrawn. Some very transparent varieties, like Englypha, become almost invisible in glycerine jelly. These I mount either in some aqueous medium or, still better, in CS_2 or other medium of high refractive index, in which the beautiful plated structure of Englypha is very clearly displayed. It has been suggested to me to arrange the shells in some artistic design ; but while in some cases the result would be very pretty, yet many species are so varying and unsymmetrical in form that a strewn slide containing the various varieties and the transitional forms would be easier of preparation and of much more scientific value.

THE AMMONIACAL FERMENTATION OF URINE.

VERANUS A. MOORE, M. D., Washington, D. C.

The fact is well known that urine undergoes putrefactive changes when it is allowed to stand for a considerable length of time at the ordinary temperature. In following these in the order of their occurrence we find that the clear urine first becomes faintly, then heavily clouded, and finally, after several weeks standing, it clears with the formation of a considerable quantity of a grayish, more or less granular, sediment. The color of the cleared urine is always several shades darker than that of the fresh liquid. A further examination shows that its acid reaction is converted into an alkaline one simultaneously with the occurrence of the clouded appearance, and with this change in its chemical reaction the odor becomes penetrating and ammoniacal in character. The source of the ammonia was explained by the discovery of urea, and later investigations made by Prout showed that the urea was converted into carbonate of ammonia by a process of hydration. By "ammoniacal fermentation of urine," therefore, is meant that change in urine by which its urea is transformed into carbonate of ammonia.

The careful chemical investigations that have been made during the past two centuries for the purpose of determining the cause of this transformation of urea into carbonate of ammonia have been negative in their results. Van Helmont, in the seventeenth century (1682), believed the cause to be the action of some putrefactive ferment, the exact nature of which he did not know. He was supported in this theory by other investigators following him; but a satisfactory explanation of this phenomena was not made until the time of Pasteur, who showed that the "ammoniacal fermentation" was due to the presence of an organized ferment (micro-organisms), and thus he transferred the line of investigation of this subject from a purely chemical to a biological basis.

The bacteriological investigations of this subject by Pasteur, Van Tieghem, Miquel, Leube, and others during the last thirty years have been so confirmatory that no further evidence is necessary to establish the theory of an organized ferment advanced by Pasteur. I have repeatedly preserved urine for several months in apparently a perfectly fresh condition by first sterilizing it by discontinuous heating for several days at a temperature of 65° C., after which it was continuously exposed to a temperature most favorable for the development of ammoniacal fermentation; also by placing it unsterilized in a temperature below that necessary for the development of bacteria. The results of many carefully conducted experiments warrant the statement that without the development of certain bacteria urine will remain unchanged although subjected to all the other influences necessary for this decomposition. As experiments have shown that the urine within the healthy bladder of people not suffering from bacterial disease is germ free, it is evident that the organisms producing this decomposition are to be sought for among the external surroundings or in the urethral canal, where they have entered either by accident or where, when once introduced, they remain in accordance with some as yet unknown law of localization. Their presence in this canal is shown from the fact that urine passed directly into sterile flasks without external contamination will undergo decomposition; but if the urethral canal is previously disinfected with some antiseptic solution it will remain unchanged for an indefinite time.

Biological History.

In 1860 Pasteur (1) showed that in decomposing urine the transformation of the urea into carbonate of ammonia was due to a micrococcus which he found and designated as *Torula ammoniacale.* A few years later Van Tieghem (2), in his inaugural thesis, presented to the "Faculty of Sciences of Paris," considered the cause of ammoniacal fomentation to be a micrococcus (spherical globules), which arranged themselves in long chains (streptococci).

Twelve years later (1876) Pasteur (3) recognized streptococci in decomposed urine, but says: "They must not be confounded with the ferment of urine, which they resemble in the diameter of their grains. The ferment of urine is formed of *couples de grains*" (diplococci). Whether the streptococcus of Van Tieghem and the diplococcus of Pasteur are identical or not we will not attempt to decide. The crude condition of bacteriology at that time, in the presence of subsequently developed facts, will admit of their being considered identical species, notwithstanding the discrepancies in the original descriptions. Professor Leube and P. Miquel have considered them as belonging to the same species.

In 1879 P. Miquel (4) isolated a rod-shaped germ from decomposed urine which, when introduced into sterile urine, would cause it to undergo ammoniacal fermentation. He gave it the name *Bacillus urea.*

Six years later (1885) Professor W. Leube (5) published an article upon "The Ammoniacal Fermentation of Urine," in which he describes three rod-shaped germs and one micrococcus isolated from urine and a sarcina from the saliva that would transform urea into carbonate of ammonia. The rod-shaped germ first described he called *Bacterium urea.* The other two are not specifically named other than by number. The micrococcus he considers to be the same as the one studied and described by Pasteur and Van Tieghem.

Flügge (6) describes a micrococcus that was isolated in his laboratory and which produced the same effect on urine as the micrococcus described by Pasteur. On account of its power to liquefy gelatine it was called *Micrococcus urea liquefacinas.*

In 1889 P. Miquel (7), in an article on "Ferments of Urine," describes six bacilli (Bacillus urea φ, B. u. β, B. u. γ, B. ε, B. ϐ, and B. ρ), five micrococci (Micrococcus urea φ, M. u. β, M. u. γ, M. u. ϐ, and M. u. ε), and one sarcina (Sarcina φ), which would transform urea into carbonate of ammonia. The *Bacillus urea β* is the same as that described by him in 1879, and he considers it the same as the *Bacterium urea* of Leube. He quotes from several investigators who have conceded to him the priority in the discovery of this bacillus The micrococcus urea φ is the same as the micrococcus urea of Leube, and supposed to be the same as the micrococcus studied by Pasteur and Van Tiegheur. The sarcina is the same as that described by Leube. To summarize, we have as organized ferments of urine already described six micrococci (Pasteur's, Flügge's, and Miquel's), seven bacilli (eight if Miquel's *Bacillus urea β* and Leube's *Bacterium urea* are not indentical), and one sarcina that are more or less active in the transformation of fresh into ammoniacal urine.

In considering the source of the organisms, we find that Pasteur, Van Tieghem, and Leube isolated their germs from decomposed urine. Flügge does not give the material from which he obtained his mic. urea liquefacians. Miquel found the germs he described in the atmosphere, soil, and water. Pasteur also found his *Torula ammoniacale* among the germs isolated from the atmosphere.

In view of these facts, I have confined my investigations thus far principally to those forms of bacteria that are to be found within the healthy urethral canal. The object of such a limit was to determine, if possible, whether or not there are certain bacteria that are constantly present in this canal which act as exciting agents in the transformation of fresh into ammoniacal urine. If this question can be definitely settled it may be effectual in explaining the causation of certain cases of cystitis which are rapidly developed after the introduction of bougies and catheters. Dr. Ricard (8) finds that if these instruments are made aseptic that their introduction is harmless, providing the urethral canal is not infected; but if the urethral canal is already infected, it must previously be washed with a saturated solution of boric acid in order to avoid a subsequent more or less acute cystitis. It seems reasonable to suppose that if germs are found constantly within the healthy urethra that are active in prodncing ammoniacal urine that this canal as well as the instruments should always be thoroughly disinfected before any operation is attempted.

Methods Employed in Collecting and Examining Urine.

The methods employed both in collecting the fresh urine and in its subsequent examination are of much importance and worthy of careful consideration. The following precautions were rigorously adhered to in collecting the fresh liquid: The glands and external urinary meatus were carefully disinfected with either a 5 per cent. solution of carbolic acid or a 1–1000 solution of corrosive sublimate. The urine was passed directly into a sterilized Erlenmeyer flask, the mouth of which was previously flamed and the cotton-wool stopper replaced immediately after urination. Care was also taken to avoid dust and currents of air. A few cubic centimeters were immediately removed from the flask by means of a sterile pipette for a preliminary chemical examination. Only normal acid urine was retained for the bacteriological examination.

For the isolation of the germs the well-known Koch gelatine-plate method was employed. Esmarch rolls were at first tried, but on account of the presence of liquefying germs they were abandoned. In several cases a plate culture was made from the fresh urine by adding from ¼ to 1 c. c. of it to a tube of liquid gelatine by means of a sterilized pipette, and after thoroughly mixing, pouring the gelatine upon sterile glass plates. These were kept at a temperature of 65 to 75° F. These plates developed from one to four colonies, occasionally more, often none at all, showing how comparatively few bacteria there are in freshly voided urine. The flasks were allowed to stand at the ordinary temperature (65° to 75° F.) for from 24 to 48 hours, when the urine became clouded and either neutral or alkaline in reaction. A microscopical examination at this time showed a greater or less number of bacteria present. Plate cultures in gelatine were then made. One small platinum wire loop (holding from $\frac{1}{60}$ to $\frac{1}{50}$ c. c.) of the fermenting urine was added to a tube containing about 10 c. c. of liquid gelatine. After thoroughly mixing, from 2 to 4 loops of the first gelatine were added to a second tube of gelatine. The gelatine was poured on sterile plates. (These were carefully protected from external contamination by means of flamed bell jars. Sterile "double-dishes" were also used.) They were kept at a temperature varying from 65° to 75° F. The number of loops taken for the first tube of gelatine depended upon the number of germs seen in the microscopical examination. When the germs were exceedingly numerous the first dilution was made in a tube containing about 7 c. c. of sterile distilled water. The first plate usually developed innumerable colonies after 24 to 48 hours. The second plate as a rule developed from 50 to 300 colonies. As soon as they were sufficiently developed they were examined both *macroscopically* and *microscopically* and a subculture made in sterilized urine and gelatine from a colony of each apparently different form. The plate cultures were repeated after the urine had reached its maximum degree of alkalinity, and again after it had been standing for from 2 to 3 weeks. In a few cases parallel plates in agar were made, but no germ was found to develop in this medium that did not grow on the gelatine. The subcultures were carefully examined, and all those in which the urine culture became alkaline were retained for more careful study. By this process I have isolated from urine about 12 forms of bacteria, mostly micrococci, of which five, on account of their property of producing ammoniacal fermentation in urine, will receive a somewhat detailed description. Of the remaining forms, one was a strepticoccus which was present in every specimen of urine examined, two were bacilli, and the remainder were micrococci. The bacilli were very rarely found.

Culture Media Employed in Testing the Reaction of These Germs.

For a preliminary examination of these germs the ordinary culture media were employed, but in order to test their power to transform urea into carbonate of ammonia, media containing that substance were necessarily resorted to. Of those sterilized normal urine seemed to be the most practical, as well as to afford a sufficient test for the property of the germs in question. Other media, such as acid-beef-infusion-peptone and gelatine plus urea, were also employed. Of the ordinary media, nutritive gelatine was the most important from a differential standpoint.

(1) *Sterile urine:* This was prepared by distributing fresh acid urine in sterile culture tubes, about 10 c. c. in each tube. As the urea is broken up by a high temperature the tubes were placed in a large water-bath and heat to a temperature of 65° C. for two hours each day for four consecutive days. Their sterility was tested by allowing them to stand for several days in an incubator at a temperature of 36° C. The urine remained in every instance clear and acid in reaction.

(2) *Acid-bouillon plus urea:* This was prepared by macerating 100 grams of finely chopped beef (freed from fat) in 200 c. c. of distilled water. After standing for from 18 to 24 hours in a cool place it was strained through a coarse linen, and to the simple beef-infusion ½ per cent. sodium chloride and 1 per cent. peptone were added. It was then boiled for thirty minutes, cooled, filtered, and ½ per cent. urea added. The liquid was then distributed in sterile culture tubes and sterilized by heating at 65° C. for three hours each day for four consecutive days. It remained clear and acid after standing for several days at a temperature of 36° C.

(3) *Acid gelatine plus urea:* This is prepared precisely in the same manner as the acid bouillon, with the addition of 10 per cent. gelatine, which is added before the beef-infusion is boiled. The urea is added to the liquid gelatine after it is filtered. It is preserved in sterile cotton-plugged test tubes and sterilized by the same method as the bouillon, after which it remained clear and acid in reaction.

Description of New Germs.

In the light that we already have concerning this class of organisms and in the present unsettled condition of bacteriological classification, it seems more desirable to omit the assigning of specific names to the micrococci about to be described, but simply differentiate them with reference to their morphological characters and their growth in certain culture media. They will, therefore, be designated by the first letters of the alphabet, printed in small capitals, as *micrococcus* A, mic. B, etc. In comparing the morphological and cultural characters of these germs with the description of the micrococci described by Pasteur, Flügge, and Miquel I have failed to find sufficient resemblances to establish the identity of any of these micrococci with those heretofore described. Their effect upon urea in nutrient solutions is, however, practically the same.

Micrococcus A.—A micrococcus varying in diameter from 0.6 μ to 1.2 μ; average, 0.9 μ. When grown in bouillon the cocci appear most usually in small clumps and in the form of diplococci. There seems to be a marked disposition in the cocci to be closely united in pairs. In acid bouillon they are frequently observed united in short chains.

Agar (surface growth) at 36° C.—After 24 hours round, convex, grayish colonies appear with a glistening surface and sharply defined border. When they are separated by a distance of one-half inch or more they are about 1 millimeter in diameter. After growing for several days the colonies become opaque, whitish, but otherwise unchanged in appearance. The colonies are quite brittle. The growth in the condensation water is quite vigorous, giving it a clouded appearance.

Gelatine (needle culture).—In nutrient gelatine the growth is quite vigorous at 70° F. Along the needle track, after 24 hours, a grayish, opaque, somewhat granular line appears, which consists of crowded colonies. At the surface the growth is more vigorous, forming an elevated glistening band around the needle puncture. This may be due to the greater number of germs collected there. On the second or third day the gelatine begins to liquefy along the entire length of the needle track. At first the liquefaction is generally more rapid on the surface, giving a cone-like appearance to the liquid portion, the base being at the surface (Pl. 1, Fig. 1). Very often the liquefaction is in the form of a cylinder. The entire mass of gelatine is liquefied in from 8 to 20 days. In a higher temperature (75° to 80° F.) the growth is more rapid. The liquid gelatine holds in suspension fine granular masses of germs $\frac{1}{10}$ to $\frac{1}{4}$ millimeter in diameter. A considerable quantity of a grayish sediment collects in the bottom of the tube; also a fine granular pellicle forms over the surface. Both the pellicle and sediment are very viscid.

The liquefied gelatine will not set, showing that it has become peptonized.

In alkaline bouillon the growth is very feeble, the liquid becoming barely clouded, even after several days standing. In acid bouillon the growth is at first also feeble, but on the second day it is very much increased, the liquid is quite clouded, and a granular pellicle forms over the surface. The acid reaction is not appreciably changed. In sterile milk at 36° C. this germ coagulates the casein after about two weeks standing. It grows very vigorously. The reaction remains slightly acid. A slight sour-milk odor is emitted.

Sterile urine at 36° C. This germ multiplies very rapidly in this liquid. After 18 hours it becomes quite clouded with the formation of a grayish sediment. Its acid reaction is changed to a feebly alkaline one, which becomes very strong after 36 to 48 hours. The odor is penetrating, resembling that emitted by ammonia water. In acid bouillon plus urea at 36° C. this germ grows very rapidly. After 24 hours the liquid is very clouded, with considerable sediment formed in the bottom of the tube. The reaction is changed to a strong alkaline one. A slight ammoniacal odor is emitted. In the acid gelatine plus urea the growth is more vigorous and the liquefaction more rapid than in the ordinary gelatine. With the liquefaction its reaction is changed to a strong alkaline one. The liquid gelatine becomes perfectly cleared, with a somewhat viscid sediment.

This germ is easily stained with the aniline dyes. It does not take the Gram stain.

Micrococcus B.—A micrococcus varying from 1.0 μ to 1.8 μ in diameter; average, 1.4 μ. It occurs single, in pairs, and small clumps. It is occasionally seen in short chains, especially when grown on agar or gelatine.

Agar (surface growth) at 36° C.—The growth of this germ on agar resembles in its general appearance that of the micrococcus A. It differs from that germ in two respects: (1) The growth on the surface is very viscid. (2) The condensation water remains clear, with a grayish, somewhat viscid growth at the bottom.

Gelatine (needle culture) at 75° F.—After 24 hours a grayish granular line appears along the needle track. At the surface the growth is more vigorous, spreading and liquefying the gelatine in a thin layer over the entire surface. The growth along the needle track does not increase, but the gelatine gradually liquefies from the surface downward until from one-half to two-thirds of the gelatine has become liquid. The liquefaction does not extend beyond that point (Pl. I, Fig. 2). A more or less thick, whitish membrane forms over the central portion of the surface, rarely forming a complete pellicle. A layer of grayish, viscid growth forms over the surface of the non-liquefied gelatine. The liquid portion holds in suspension grayish granules, which are occasionally so fine as to give a uniform clouded appearance to the liquid gelatine. The liquefaction is completed in from one to two weeks.

In bouillon at 36° C. the growth is very feeble. After 24 hours the liquid is very faintly clouded. In from two to four days a somewhat stringy growth appears in the upper part of the liquid; also granular masses, varying from 1 to 2 millimeters in diameter. These are quite viscid. In acid bouillon, after several days a few granular masses are formed on the surface; also a whitish deposit on the sides of the tube at the surface of the liquid.

In sterile milk this germ, after about two weeks, coagulates the casein, leaving a clear supernatant liquid.

Sterile urine at 36° C.—This germ grows rather feebly in urine. After 24 hours the liquid is very faintly clouded and neutral in reaction. After 48 hours it is strongly alkaline and clouded with the formation of a small quantity of sediment. In acid bouillon plus urea the growth is not vigorous. After 24 hours the reaction is feebly alkaline. At the end of two to three days the reaction is strongly alkaline and the liquid quite cloudy—a slight penetrating odor. In acid gelatine containing urea the growth is more vigorous than in the ordinary gelatine. The liquefied gelatine is strongly alkaline in reaction.

This germ is easily stained with the aniline dyes. It takes the Gram stain.

Micrococcus C.—A micrococcus varying in diameter from 0.8 μ to 1.5 μ; average, 1.2 μ. It occurs single, in short chains, and small clumps. There are also diplococci forms.

Agar (surface growth) at 36° C.—On this medium the growth does not differ appreciably from that of micrococcus A.

Gelatine.—This germ liquefies gelatine very slightly, so that the growth, which at first is upon the surface, after several days becomes slightly depressed, never more than one to two millimeters. The growth along the needle puncture is whitish, opaque, viscid, and quite thick. The colonies along the needle track are very small. They do not liquefy the gelatine. (Pl. I, Fig. 3.)

In bouillon the growth is very feeble; after 24 hours the liquid becomes faintly and uniformly clouded. There is no further development. Sterile milk inoculated with this germ and kept at a temperature of 36° C. becomes thickened in the lower half of the tube after about six days. There is no separation of the casein from the aqueous portion; no odor; slight acid reaction.

Sterile urine.—In this liquid the growth is feeble. On the day following its inoculation the reaction is faintly alkaline, the liquid barely clouded. On the third day there is a thin granular deposit on the sides of the tube, and the reaction is strongly alkaline, ammoniacal odor. In acid bouillon plus urea at 36° C. the liquid becomes clouded on the day following the inoculation, with the formation of a considerable quantity of sediment; strong alkaline reaction, penetrating odor. After ten days standing it becomes clear, with a sediment. It remains strongly alkaline. In acid gelatine plus urea the growth is very feeble; no liquefaction.

It is easily stained by the ordinary methods. When stained according to Gram a great majority of the cocci take the brown stain perfectly. A few (about one in fifteen) retain the blue stain. These are of the maximum size (1.5 μ).

Micrococcus D.—A micrococcus varying in diameter from 0.7 μ to 1.2 μ; average, 0.9 μ. It grows most usually in small clumps and in pairs. It is very rarely seen single.

Agar (surface growth) at 36° C.—When not crowded the isolated colonies, after 24 hours, are from 1 to 1½ millimeters in diameter, round, flat, with a smooth border and sharply defined margin; A considerable growth in the condensation water. The colonies at first are grayish white, glistening, but after several days they become a light cadmium yellow. The size is very slightly increased. When grown on agar containing 5 per cent. glycerine the growth is more feeble, but the color more intense.

Gelatine (needle cultures).—Along the needle track the colonies develop after 24 hours as minute grayish points. After several days they attain to a diameter of ⅛ to ¼ millimeter, spherical, granular, yellowish. On the surface the growth is spreading, glistening, of a deep cadmium yellow. The growth about the needle puncture appears in more or less concentric bands. The outer bands also have a radiating appearance, produced by alternate lighter and darker lines extending outward from the more central portion. After about two weeks the growth becomes slightly depressed below the surface (Pl. I, Fig. 4).

In bouillon the growth is feeble. After 48 hours, at 36° C. the liquid becomes clouded in the upper portion of the tube, with a small quantity of a grayish deposit formed on the side of the tube. It produces no change in the appearance of milk.

Sterile urine.—In this liquid the growth is abundant; the liquid is heavily clouded after 24 hours; the reactions strongly alkaline. After from two to three days a considerable quantity of sediment is formed in the bottom of the tube. This germ is the most rapid in its transformation of acid into alkaline urine of any of the germs here described. Under its influence acid urine will become decidedly alkaline in eighteen hours. In acid bouillon containing urea the growth is vigorous. In 24 hours the liquid is very cloudy and strongly alkaline in reaction. In acid gelatine plus urea the growth is more feeble than on the ordinary gelatine. It takes the Gram stain.

Micrococcus E.—A micrococcus varying from 0.9 μ to 1.5 μ in diameter; average, 1.2 μ. It occurs most usually in small clumps and in pairs. It is frequently found single, often in short chains.

Agar (surface growth) at 36° C.—After 25 hours quite fleshy, round, convex colonies appear, 1 millimeter in diameter when not crowded. They are of a grayish yellow color, apaque. After several days the growth becomes a much deeper yellow. The condensation water is clear, with a viscid, flaky growth at the bottom. The surface growth is not viscid.

Gelatine.—The growth in this medium resembles that of the micrococcus B. It differs from it in the color of the membrane and sediment, which are cadmium yellow. (Pl. 1, Fig. 5.)

In bouillon the growth is feeble. It does not produce any apparent change in the appearance of milk.

Sterile urine.—The growth in this liquid is very slow. On the second day after inoculation it becomes feebly alkaline, with the formation of a granular deposit on the sides of the tube. In acid bouillon containing urea the growth is more vigorous. After one day, at 36° C. the reaction becomes strongly alkaline, emitting a penetrating odor. It liquefies acid gelatine containing urea very slowly. The liquefaction does not extend more than three to five millimeters below the surface. The liquid gelatine is strongly alkaline in reaction. This germ is readily stained by the ordinary methods. It takes the Gram stain.

The characteristic properties of these micrococci, especially their reaction on nutrient gelatine, are more or less modified by cultivation. Their power to break up urea, however, does not seem to be diminished.

I have examined, after the methods previously described, twelve specimens of urine. Of these seven contained only the micrococcus A, two the micrococcus C, one the miccrococci A and B, one the micrococci B and D, and one the micrococci A, C, and E. Eight of the specimens were taken from the same individual, six of which contained (of the germs described) the micrococcus A only, and two the micrococcus C. The other four specimens were obtained from four other individuals, one of which contained only the micrococcus A.

In addition to the above, one specimen of urine from a person suffering with cystitis was examined. The urine was clouded, strongly alkaline in reaction, and contained a considerable quantity of crystals, principally triple phosphates, pus cells, and amorphous salts. A large number of micrococci were seen in a coverglass preparation. The bacteriological examination showed that a large number of these were the micrococcus A. Thus far this is the only specimen of urine from an authentic case of cystitis that I have been able to obtain in a condition suitable for a bacteriological examination.

I have also isolated several germs from external sources that are equally as active in the production of ammoniacal fermentation as those already described. Two of these, a bacillus and micrococcus, were obtained from decomposed urine that was previously sterilized and then exposed to the atmosphere of the laboratory by removing the cotton-wool stoppers from the tubes containing it. The sterile, acid urine became clouded and alkaline in from three to five days. A second bacillus was obtained by inoculating a tube of sterile urine with several drops (about 1 c. c.) of Potomac water and allowing it to stand in an incubator, where it became clouded and alkaline in twenty-four hours. The bacillus was isolated from the other germs by means of plate cultures. A micrococcus was also found in saliva which is arranged in fours (tetracoccus), that is very active in transforming urea into carbonate of ammonia. These germs have, as yet, not been carefully studied, and their isolation is mentioned here only to illustrate the general distribution of bacteria possessing this property as well as to confirm similar statements made by other workers in this line.

Soluble Ferments.

In the last decade Musculus (9) announced the discovery of a soluble ferment which he obtained from ammoniacal urine that would, when introduced into fresh urine, transform its urea into carbonate of ammonia. The name *urase* was proposed for this substance, as it was supposed to belong to the same class of ferments as ptyaline diastase, etc. The method of isolating this ferment was reported by Musculus to be one of simple precipitation. The results, however, that have been reported by others who have attempted to isolate this substance have been, so far as I can learn, negative in character. P. Miquel, after a long series of experiments, in many of which the method of Musculus was carefully carried out, concludes with Prof. Leube, that the existence of a soluble ferment in urine is not sufficiently proven to be accepted without further demonstration. Thus far I have made no experiments for the purpose of isolating a soluble ferment. Whether there is such a ferment or not will, together with many other interesting questions that these preliminary investigations have suggested, have to remain unanswered until other and more extended investigations shall be made.

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PLATE I.

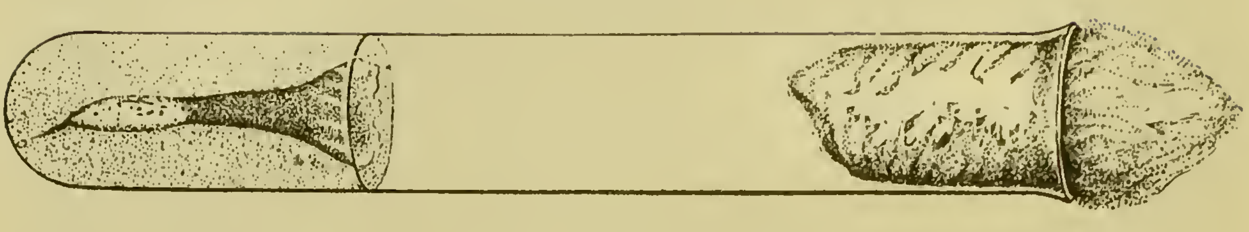

FIG. 1.

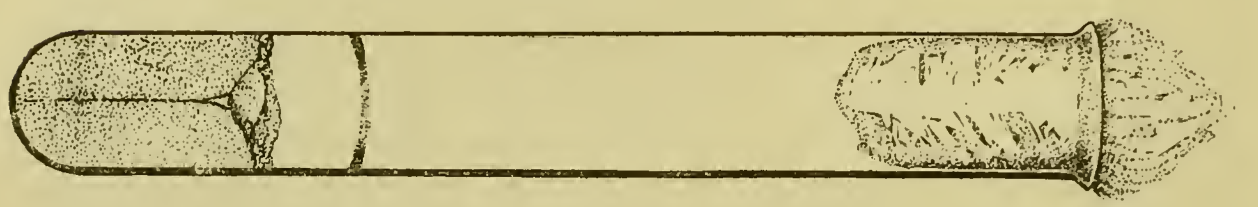

FIG. 2.

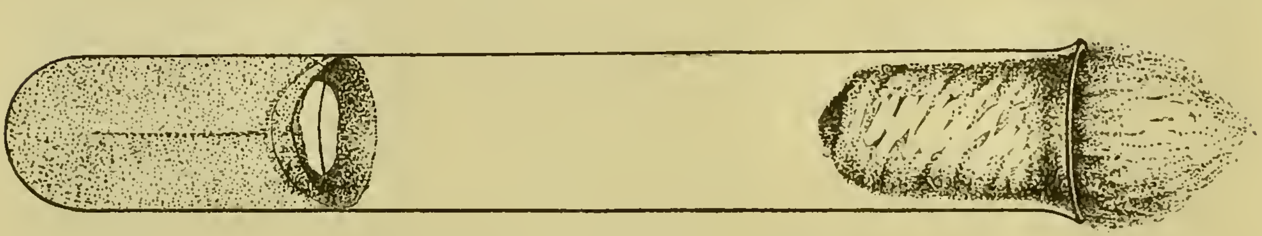

FIG. 3.

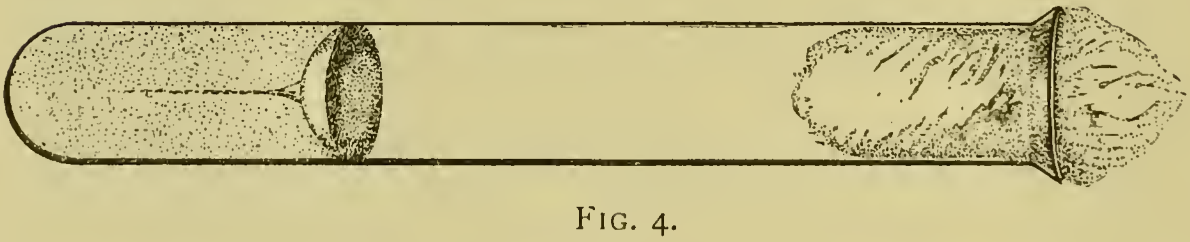

FIG. 4.

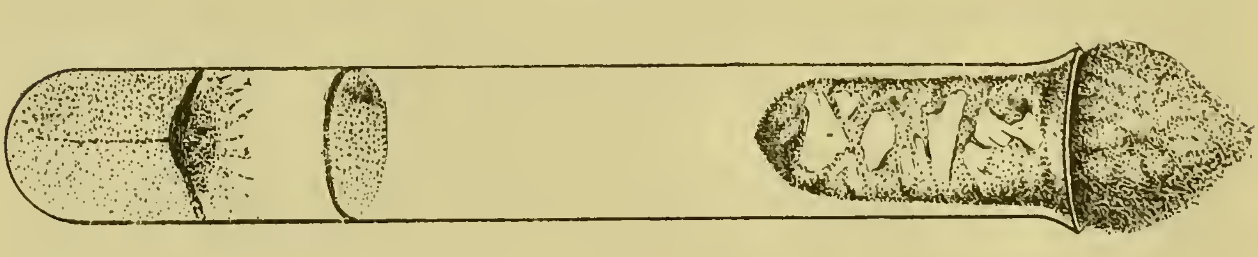

FIG. 5.

Description of Plates.

Plate I.

Gelatine needle cultures of the micrococci described.

Fig. 1. Micrococcus A—Culture seven days old.
" 2. " B " " "
" 3. " C " six weeks old.
" 4. " D " " "
" 5. " E " " "

Plate II.

Drawings made from cover-glass preparations of urine cultures, one day old, of the micrococci described. The preparations were dried, passed three times through a flame, and stained for a short time with alkaline-methylene-blue. The position of the germs in the field was determined by means of Abbé's camera lucida, Zeiss 2 mm. apochromatic lens, No. 4 eye-piece.

Fig. 1. Micrococcus A.
" 2. " B.
" 3. " C.
" 4. " D.
" 5. " E.

PLATE II.

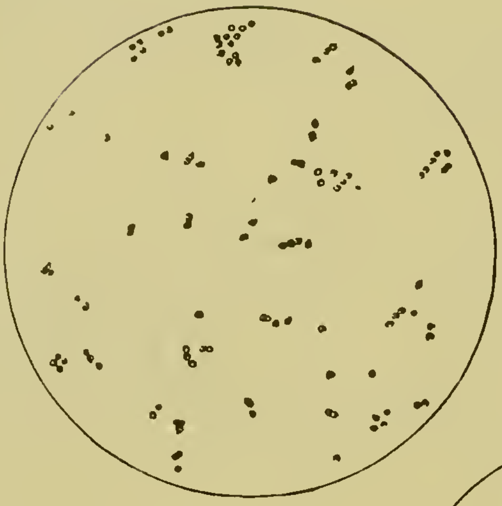

Fig. 1

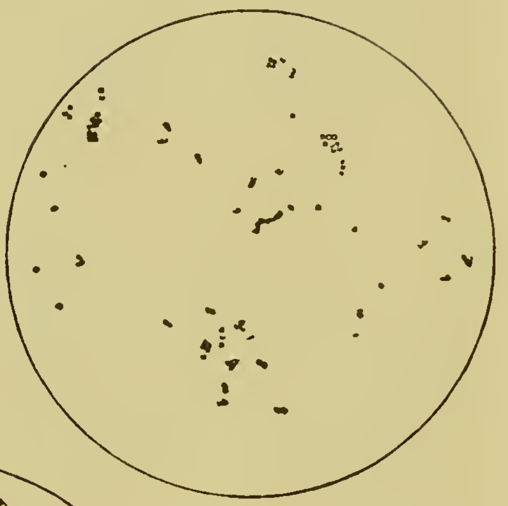

Fig. 2

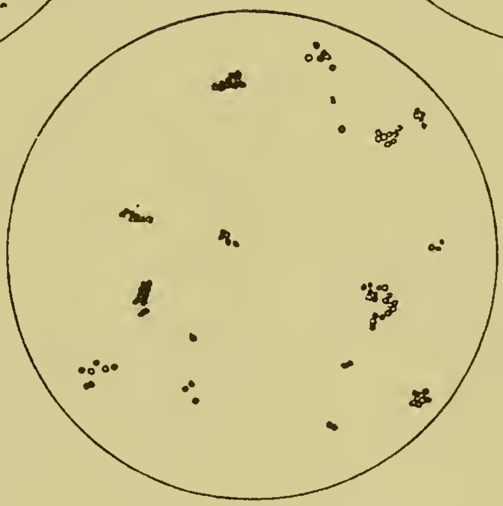

Fig. 3.

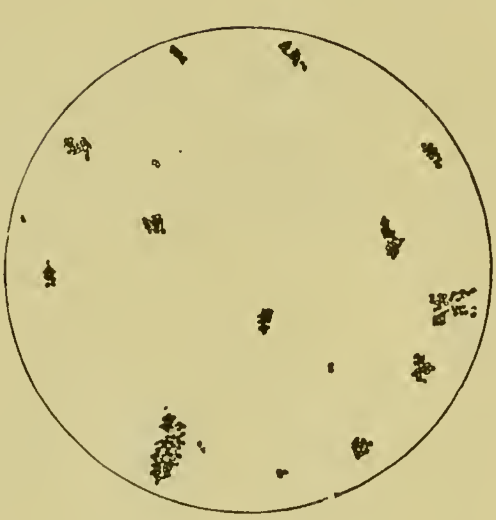

Fig. 4

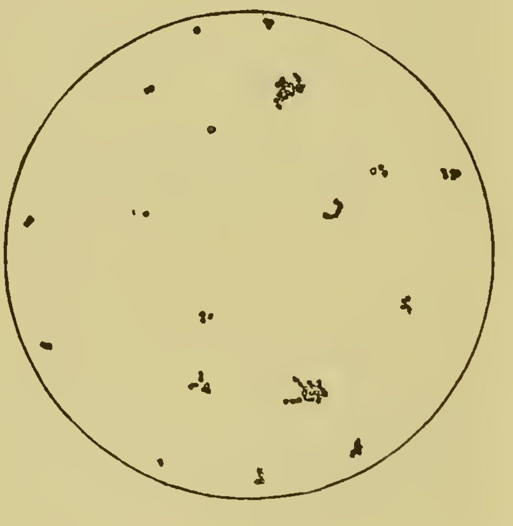

Fig. 5

Haines, Del.

IMBEDDING AND SECTIONING MATURE SEEDS.

Willard W. Rowlee, Ithaca, New York.

The modifications that may be made of the paraffin method of imbedding objects for sectioning are very many. There is always some danger, however, of shrinking delicate and very soft plant tissue. This is due to the use of heat for the process of infiltration, and probably some of the non-heat-employing methods will be found preferable where such delicate tissue is to be imbedded; but, for objects that will withstand this process of infiltration, the paraffin method has many advantages over other methods. Imbedded in paraffin, objects are held firmly and may be preserved as long as desired without further attention.

For imbedding mature seeds I have found nothing that was equal to paraffin. The texture of the seed is often very dense and offers much resistance to a knife. For this reason I found it better to use the harder grade of paraffin. A second serious difficulty that was met with in imbedding seeds was the fact that there was little if any tissue connecting the embryo* with the seed coats. Thus it would too often occur that just as the sections were being taken through the middle of the seed (the most valuable ones are those near the center) the embryo would leave the coats and the whole series would be spoiled. The inner surface of the inner coat in many seeds is highly polished, and as soon as there is nothing to retain the embryo but its adhesion to the coat it will loosen. The paraffin does not seem to hold the two together as might be expected. It was suggested that, in order to soften the tissue and thereby make it more susceptible of infiltration, it would be well to thoroughly soak the seeds in water before hardening in alcohol. This was tried and there was a great improvement in the results. Fewer of the sections went to pieces after they were transferred to the slide and the parts of the seed kept their respective positions much better.

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* The term "embryo" is used here where on some accounts it would be better to use the word nucleus. The embryo is often but a very small part of the substance contained within the seed coats. If the word nucleus is used it might be confused with the histological significance of the term.

In order to study the microscopic structure of seeds, much more satisfactory results can be obtained if the sections are kept in series. It is often necessary to have two or more successive sections before a correct idea of the seed can be obtained.

The method used is a modification of the one used and taught in the histological laboratories of Cornell University. In its practical application it is as follows: In choosing seeds to section, great care is taken to get those which are well filled. This precaution is especially important, as many seeds never develop more than the coats or the enveloping ovary coats. If a seed has a straight embryo, or even a bent one, it is better to determine by dissection just how the parts of the embryo are arranged, with reference to the external parts or form of the seed. Thus the seeds of "Helianthus tuberosus" are flattened and slightly wedge-shaped. The embryo within is straight and the upper or inner surface of the cotyledons lie in a plane parallel to the plane in which the seed is flattened. Moreover, the cotyledons are in the broader, upper end of the seed. Where the seed has no external character, as in a "Eupatorium," by which the position of its internal parts may be located, one has either to take the chances of getting the sections in the right plane or open the coats enough to see how the parts are arranged, and then mark the seed in some way. Having selected well-filled seed, I put them in water at the ordinary temperature of the laboratory for 36 hours. From the water they are transferred immediately to weak alcohol (40 per cent.), and gradually hardened by transferring to stronger alcohol until they are in 95 per cent. alcohol. Schultze's apparatus may be used to advantage in hardening. Next they are transferred to equal parts of alcohol and chloroform for from four to eight hours, the time depending on the size of the seeds, then in pure chloroform for the same length of time; then for twenty-four hours into chloroform with as much paraffin as it will dissolve at the ordinary temperature. From this into paraffin softened with chloroform until the melting point is about 35° C. The specimens are kept in this melted paraffin twenty-four hours. I have always been careful not to let the temperature go above 47° C., although I think it probable that a somewhat higher temperature would not hurt the tissue of a seed. From this the seed may be imbedded in hard paraffin and will be found to be thoroughly infiltrated.

The seeds may be sectioned in the paraffin blocks either freehand or with a microtome. It is highly essential that the sections be kept in series, and that none of them should be missing. The texture of a seed is so fragile that when cut in thin sections the least carelessness may spoil a section.

A very effectual way to keep sections intact when they are cut in a paraffin block is that proposed by Dr. Mark ("American Nat.," 1885, p. 628). It consists in collodionizing the object as the sections are taken. Very thin collodion should be used and applied to the cut surface after each section is taken. Lee ("Vade Mecum," p. 150) recommends that "the collodion be of such a consistency that when applied to the surface of paraffin it will dry in two or three seconds. This has no tendency to cause sections to roll. * * * As soon as the collodion is dry, which ought to be in two or three seconds, cut the section, withdraw the knife, and pass the collodionized brush over the newly cut surfaces of paraffin." The section is placed collodion side down on the slide. The sections are fastened by placing a little clove-oil collodion on the slide and placing the sections in it, and then evaporate off the clove-oil. The sections are placed on the slide in series and in definite order. They are then washed in xylol for ten minutes or more. This removes the paraffin. Then they are washed with alcohol, afterwards with water, and stained. I have found no stain as good as hæmatoxylin for this work. They should be stained with it from three to five minutes. After washing the staining agent away with water, dehydrate with alcohol and clear. Three parts turpentine and two parts carbolic acid make a very good clearing mixture. Canada balsam dissolved in xylol is used for mounting.

In sections thus prepared one can distinguish without difficulty, in Shepherd's Purse, Golden Rod, or any endospermous seed, the coats, the plumule, composed as is the lower tip of the radicle, of small thin-walled cells bearing nuclei. These two regions of growth are connected by slightly elongated cells which are also thin-walled. The larger cells making up the tissue of the cotyledons are stored with food. In many seeds a trace of a vascular system may also be seen ; also the peculiar arrangement and markings of the cells composing the coats.

Seeds differ so much that one would need to make many variations in method to suit special cases ; but as a general method I have found this to be a success, and I believe the histology of any seed may be demonstrated by applying it.

SOME METHODS OF TREATING NERVE TISSUES.

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The aims to be sought after in the study of microscopy are not alone those looking to the perfection of the instrument nor those which bear upon the selection of the specimen. The preparation of this specimen, and more especially the method of staining, is as important as the specimen itself. American microscopists are more intent upon the former ; the European microscopists pay more attention to the latter. Careful study and experimentation with the different dyes have led to the compounding of stains which have enabled the microscope to reveal many of the mysteries of histology and pathology. The introduction of the aniline dyes and their successful employment by Koch lad to the discovery of the bacillus of tuberculosis, although the existence of this organism had been prophesied by writers years before its discovery. Golgi's silver-nitrate method has advanced our knowledge of the ganglion cells of the cortex of the brain, and perhaps at some day the mystery of their poles may be revealed through some simple method of staining. The importance, therefore, of the methods of staining is not to be overlooked or undervalued in the microscopical examination of tissues, and their successful employment may lead to the discovery of new facts and data of inestimable value in advancing the present status of our science.

Among some of the recent methods employed in neuro-histology and neuro-pathology, perhaps none are so important and satisfactory as the Weigert method and the Pal modification of this method. Both methods are restricted to the examination of nerve tissues, more especially of the central nervous system, where the gradation between white and gray matter is distinct and prominent.

Both methods require hardening in Müller's fluid, or simply in a saturated solution of potassium bichromate. A recent writer in Neurologisches Centralblatt, Dr. Minor, of Moscow, finds that if sections of the brain and cord are subjected to the action of the positive pole in the bichromate solution, hardening will take place in 3, 4, or 5 days. After dehydrating and decolorizing in alcohol for some days, the preparations are ready for the imbedding mass. I have always preferred celloidin for imbedding nerve tissues and find that it is an excellent agent. It is prepared by allowing several sheets of celloidin to dissolve in equal parts of sulphuric ether and 99 per cent. alcohol. The preparations to be imbedded are placed in 99 per cent. alcohol for 24 hours, transferred to equal parts of 99 per cent. alcohol and sulphuric ether for another 24 hours, then placed for 24 hours in the celloidin solution, fastened upon corks, and are then ready for cutting.

The sections being more or less delicate and very friable, it is necessary to protect them during their passage through the various stages of the process. For this purpose collodion, photoxylin, or dextrin may be used, the *modus operandi* being as follows: Allow some of the mass to flow over a glass slide, so that a thin film remains; dry; then transfer the sections onto this prepared slide, and pour some of the mass over the sections, allowing all superfluous quantities to drip off. After a few moments the slide may be moistened in alcohol, when the sections imbedded in collodion can be removed and handled with impunity.

The Weigert method, first described by Professor Weigert, of Frankfort-a-Main, in “Fort Schritte d. Medicin,” 1884, p. 190, and 1885, p. 236; also Zeitschrift f. Wissenschaftliche Microscopie, 1885, p. 490, and 1886, p. 480, requires the sections to be placed in an aqueous-saturated solution of cuprum acetate diluted with an equal amount of water for 24 hours, in a brood oven, or 48 hours in the open air. They are then washed in 60 per cent. alcohol a few hours and placed in the Weigert staining fluid:

0.75 — 1.0	parts	hæmatoxylon.
90	“	water.
10	“	alcohol.
1	“	lithium carbonate.

At the end of 24 hours they may be removed, washed in water, and are then ready for the differentiating bath:

Borax, - - - - -	2	parts.
Ferrocyanide of potash,	2.5	“
Distilled water, - -	200	“

Some experience and great care are requisite in differentiating the sections, but with patience and judgment they can be developed with as much precision as the photographer displays in bringing out the light and shadows of a dry plate. The sections should be placed in the bath singly, so that each one may be removed as soon as it is differentiated. The length of time required is variable, depending upon the thickness of the sections. The general outlines of the white and gray matter should be known beforehand, and if pathological the seat of the lesion, so that as soon as the contour of the different regions becomes distinct the sections may be immediately removed from the bath. Overdevelopment destroys the sections, and hence as soon as they are removed from the bath they must be thoroughly washed in water for 12, 18, or 24 hours to arrest all further development. They are then dehydrated in strong alcohol, cleared in Weigert's clearing mixture—

Xylol - -	3 parts;
Carbolic acid,	1 "

Sulphate of copper, enough to cover the bottom of the bottle,

and mounted in balsam.

The gray matter, connective tissue, and vascular walls are stained light brown; the ganglion cells, dark brown, while the white matter takes on a blackish-blue or purplish-blue tint.

The Pal Method.

This method, which is merely a modification of the Weigert method, was described by Professor Pal, of Vienna, in Wiener Medizinische, Jahrbücher, 1887, p. 589. The preliminary preparation of the sections is the same as with the Weigert method, but with care and dexterity the imbedding in collodion may be dispensed with, as they are not rendered so friable and brittle. They are immediately placed in Weigert's staining fluid, to which has previously been added 3 to 5 drops of a saturated solution of lithium carbonate for every 10 c. cm. of the stain used. After 5 or 6 hours they may be removed and carefully washed in water to which some of the carbonate of lithium solution has been added. They are now placed for 10 or 15 seconds in a ¼ per cent. solution of the permanganate of potash, rinsed in 30 per cent. alcohol, and placed in the differentiating bath.

Oxalic acid, - -	1 part.
Sulphite of soda,	1 "
Distilled water,	200 "

The same hints regarding the differentiation of the sections as given under the Weigert method are applicable in this method. Should the sections be very slow in developing, they may be replaced in the permanganate of potash solution and the process continued as before. After they have been sufficiently differentiated they should be carefully washed in water for 24 hours, dehydrated in strong alcohol, cleared in Weigert's clearing mixture, and mounted in balsam.

The medullary nerve fibers are stained light blue, the neuroglia, connective tissue, and vessel walls are rendered white or yellowish white, while the ganglion cells become transparent. To stain these cells, after the development has been arrested by the water bath, the sections should be placed in a picrocarmine stain for a short time and the process completed in the ordinary way. To stain the nuclei, the borax-carmine method must be employed as a double stain. These preparations, especially after accepting the double stain, are very pleasing to the eye and more beautiful than the Weigert specimens. I have also found that they retain their stain much better than Weigert's. A serial section made over two years ago is in as good condition as ever.

The advantages which these two methods offer must be apparent to the most astute observer. To possess a method or methods which will sharply and clearly define for us the limits and boundaries of the white and gray matter of the brain and cord is a desideratum. The differentiation between the two renders the study of pathological lesions very facile, and to follow these changes cephalad or caudad becomes a very easy matter. More especially in tracing separate bundles, or even individual nerve fibers, have these methods shown their superiority over all others. The Weigert method surpasses the Pal in this respect, being more reliable and trustworthy, and yet with the latter I have traced individual fibers in the pons and medulla with surprising accuracy. Another use to which these specimens may be put is in the study of the changes which the white and gray matter undergo in the transition between cord, medulla, pons, and crura. With the aid of a magic lantern the topography of these regions can be intelligibly demonstrated to a large class, whereas formerly these parts were almost totally ignored on account of their complex and complicated structure.

For the study of angio-pathological and ganglionic changes I prefer the carmine stains to the Pal and Weigert.

PICRIC AND CHROMIC ACID FOR THE RAPID PREPARATION OF TISSUES FOR CLASSES IN HISTOLOGY.

Simon Henry Gage, Ithaca, New York.

The standard methods of hardening tissues and preparing them for sectioning require so great an expenditure of time that it is practically impossible for students in college and carrying on other university work to perform all the processes and to make any satisfactory progress in the limited time devoted to histology. Believing firmly that unless a student learns to take every individual step himself in histology, as in all other branches of sound learning, the great object is unattained, I have been experimenting for the last few years in the laboratory hoping to so shorten and modify existing methods that every step may be taken by the student himself without too great an expenditure of time. The following are the results, and they are given not because they are the best possible methods that might be used if unlimited time were at the disposal of the student, but as methods that give excellent results in a very short time.

Picric-Alcohol Method.

The hardening and fixing solution consists of equal parts of 95 per cent. alcohol and water, and to this is added 1–5th per cent. of picric acid crystals, thus:

95 per cent. ethyl alcohol - - - -	1250 cc.
Water - - - - - - - - - - -	1250 cc.
Picric acid crystals - - - - - -	5 grams.

Or for a small amount:

95 per cent. ethyl alcohol - - - -	250 cc.
Water - - - - - - - - - - -	250 cc.
Picric acid crystals - - - - - -	1 gram.

The tissue is cut into pieces of moderate size and placed in a preserving jar containing about 25 to 50 times as much of the preservative as there is tissue. It is well also to suspend the tissue or support it on absorbent cotton, or to stir the tissue around occasionally. The tissue should be left in the picric alcohol about 24 hours. If the piece is small, 12 hours will do, and an immersion of 2 to 3 days seems to do no harm. After one day the tissue is placed for 24 hours in 67 to 70 per cent. alcohol, and then for one day or longer in alcohol of from 75 to 82 per cent. It may be left indefinitely in this. Finally, just before imbedding, the tissue is dehydrated one day only in 95 per cent. or stronger alcohol. It may then be infiltrated with paraffin or collodion in the usual manner, the whole time required being 7 days, at the longest, to harden, infiltrate, and imbed a tissue ready for sectioning.

The picric-alcohol method has given excellent results for all tissues except peripheral nerves. It is especially to be recommended for organs or parts possessing ciliated epithelium. The sections may be stained with almost any color, but hæmatoxylin and carmine are especially to be commended. It is further recommended that if too much of the yellow color of the picric acid has been removed in the various processes the final dehydration be performed, in part at least, with 95 per cent. or stronger alcohol containing 1–10th per cent. of picric acid, thus:

95 per cent. alcohol - - - -	100 cc.
Picric acid crystals - - - -	1–10th gram.

The double stain of hæmatoxylin and picric acid gives very sharply defined appearances, the hæmatoxylin staining the nucleus and the picric acid the cell-body and also the ground substance somewhat.

If ammonia carmine is used as a stain, more sharply differentiated appearances are obtained by dehydrating with the following:

95 per cent. alcohol - - - - -	100 cc.
Glacial acetic acid - - - - -	1 cc.
Picric acid crystals - - - - -	$\frac{1}{10}$th gram.

Nothing has been found more satisfactory for a clearing medium than a mixture of carbolic acid crystals, 2 volumes, and turpentine, 3 volumes, thus:

Carbolic acid crystals (melted) - - -	40 cc.
Turpentine (oleum terebinthinæ) - -	60 cc.

And for a mounting medium Canada balsam, dissolved to the consistency of thick syrup in xylol or cedar-wood oil, has given excellent results.

Flemming's Chrome-Acetic Acid Method.

This has proved satisfactory for the rapid fixing of peripheral nerves and for stratified epithelia. For the stomach and intestines it has not proved so satisfactory as the picric alcohol—chromic acid, $\frac{1}{4}$th per cent; glacial acetic acid, $\frac{1}{10}$th per cent. in water, thus:

Chromic acid crystals - - - - -	6 grams.
Glacial acetic acid - - - - - -	2$\frac{4}{10}$ths cc.
Water - - - - - - - - - -	2400 cc.

The tissue is cut into pieces of moderate size and placed in 50 to 75 times its volume of the fixing agent for 12 to 24 hours. It is then washed two hours or more in water and left about 12 hours in 50 per cent. alcohol, then placed indefinitely in 75 to 82 per cent. alcohol. It may be dehydrated, infiltrated, and imbedded as described for the picric-alcohol method.

Hæmatoxylin is, on the whole, the most satisfactory stain, but the staining is not so satisfactory as after the use of picric alcohol. The staining may be hastened in this case, as in all others where it is desirable, by heating the staining agent.*

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* If the picric alcohol solution, as given above, is diluted with an equal volume of water it makes a most excellent dissociating medium for almost all the tissues. It is especially good for epithelia and for smooth and striated muscle. The striation in the striated muscle is exceedingly clear and the longitudinal fibrillation of the smooth muscle is easy to demonstrate.

For a rapid and generally applicable method for hardening tissues see also the bichloride of mercury method described by Mr. Hopkins in his article on the structure of the stomach of Amia, described later in this volume.

THE COLLODION METHOD IN BOTANY.

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In any original investigation in the field of histological botany the worker is at the outset confronted with the all-important question of how to prepare the material so that it may be available in the best possible condition for thorough systematic observation and study. The old method of making freehand sections in careful investigations is now gone by and the best histologist looks for some substance with which to infiltrate the tissue and bring it in a condition where uniform serial sections can be made. Many substances have from time to time been offered and met with varying success. Some of the more important ones that have been tried are gelatine, gelatine soap, gum, paper, shellac, wax, gum-arabic, soap, paraffin, and collodion. The last two substances have seemingly supplanted the others, and indeed they seem to offer all the advantages that can be secured by any of the others. The paraffin method, as applied to plant tissue, was published by Dr. Moll in the Bot. Gazette of January, 1888, and later many modifications have been given and the method much improved. The method has been quite extensively used, but is very long and quite disagreeable to manipulate, it often requiring 10 or 12 days to bring the tissue in proper condition for sectioning. For proper infiltrating with paraffin a temperature of from 45° to 50° C. is required, and for some of the more delicate tissues this, in my hands, has proved fatal. The method is admirably adapted, however, for many tissues that can be held in position for sectioning in no other way, as is the case with mature seeds or woody stems.

The collodion method is now coming into general use for nearly all kinds of plant tissue. For the use of collodion for infiltrating we are indebted to Duval, who first published his results in the Journ. de l'Anat., 1879, p. 185. A little later Merkel and Schiefferdecker suggested the use of celloidin, which is merely a patent collodion.

This appeared in the Arch. f. Anat. u. Phys., 1882, p. 200. Some discussion then arose regarding the relative merits of each, but it is generally agreed that one has little or no advantage over the other. The method as applied to plant tissue is a comparatively new one. In 1884, we find a short description of it in Strasburger's Botany, and some few modifications of it have since been offered. In the Histological laboratories at Cornell University, under the directions of Professors Gage and Dudley, I have tried the method, with its various modifications, on many different kinds of plant tissue, and find the following directions to be in every particular the best to obtain uniformly good results.

The tissue to be treated is first dehydrated and hardened in alcohol. For this purpose a Schultze's apparatus is of the first importance—in fact, I have found some tissue that could be hardened in no other way without shrinking. The results fully warrant the statement that no one engaged in histological botany can afford to do without such an apparatus. Many forms of it have been suggested, but for most plant tissue one after Fig. I will be found very convenient. In place

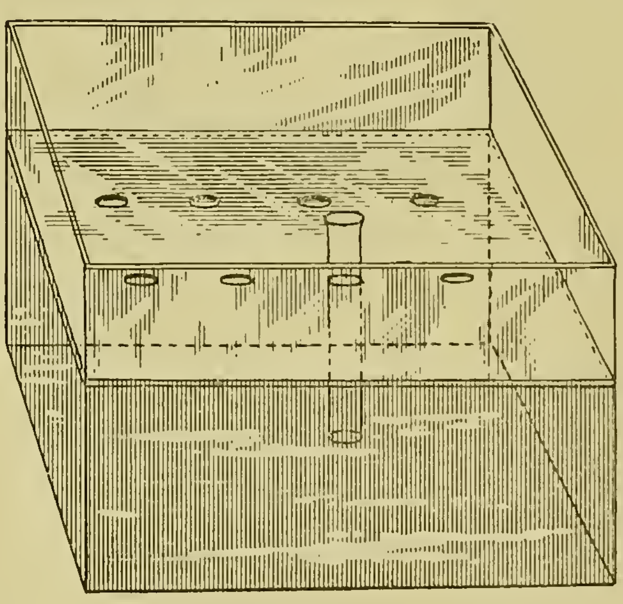

FIG. I.—Modified Form of Schultze's Apparatus,

Made by fastening into a battery jar a diaphragm of plaster of Paris which is perforated to allow to pass through the tubes provided with the chamois skin diaphragm.

A rubber band around the tubes allows them to be plunged into the jar of 95% alcohol at any desired depth.

A more convenient form of the apparatus can be made by fitting into a Whitall & Tatam museum jar, as used for another purpose in Fig. III, a diaphragm in which several dehydrating tubes can be supported at any desired level in the alcohol contained in the jar.

of the parchment diaphragm usually used, I would recommend the use of chamois skin. With such an apparatus, from 12 to 24 hours is sufficient for hardening and dehydrating any plant tissue. In using the apparatus the tissue should be packed closely in the inner tube and enough 50 per cent. alcohol added to just cover it. The tube is then placed in the 95 per cent. alcohol until the two liquids are at a level. The strength of the 95 per cent. alcohol can be kept up by adding to it from time to time some calcium chloride. This will not in any way injure the alcohol. The tissue is taken from the 95 per cent. alcohol and placed in a 2 per cent. solution of collodion, made by dissolving 2 grams of gun-cotton in 100 cc. of equal parts of sulphuric ether and 95 per cent. alcohol. In this it is allowed to remain from 12 to 24 hours, depending on the nature of the tissue, 24 hours being enough for the very firmest.

It is then transferred to a 5 per cent. solution, or the 2 per cent. solution may be allowed to evaporate until it is of the consistency of the 5 per cent. solution. The former method is more uniformly satisfactory. The tissue is allowed to remain in this solution about 12 hours. It is then taken out and arranged in position on a cork or block of wood of a convenient size to fit in the jaws of the microtome. It is not necessary that the corks be previously soaked in collodion. By means of a camel's-hair brush the material on the cork is covered with successive layers of collodion until it is quite enclosed in the mass, allowing each coat to dry slightly before applying the next. After the tissue is covered it is allowed to harden in the air a few minutes, and then placed in about 80 per cent. alcohol to harden. Much difference of opinion exists regarding the proper strength of alcohol to use for hardening the collodion, but 80 per cent. answers very well, and the tissue can be kept in it a long time without deteriorating. After a few hours the collodion will be firm enough to section. For sectioning any sliding microtome will answer, but one especially adapted for the purpose will enable one to incline the object, which can be seen clearly through the collodion, to any desired position, and sections can be taken in any plane. It is also necessary that the sections be removed with a long sweeping cut, since a direct cross-cut would tear them. The section should be kept covered with alcohol while being removed, and then floated from the knife to the slide. The slower the section is cut the better it will usually be. Serial sections can be arranged in their proper place on the slide. For fixing the sections some dry ether vapor is blown upon them. A very convenient form of apparatus for this purpose is shown in Fig. II. The ether vapor dissolves the collodion and fastens the section to the slide. The sections are then washed with water, stained, the surplus stain washed off with water, and the section dehydrated with

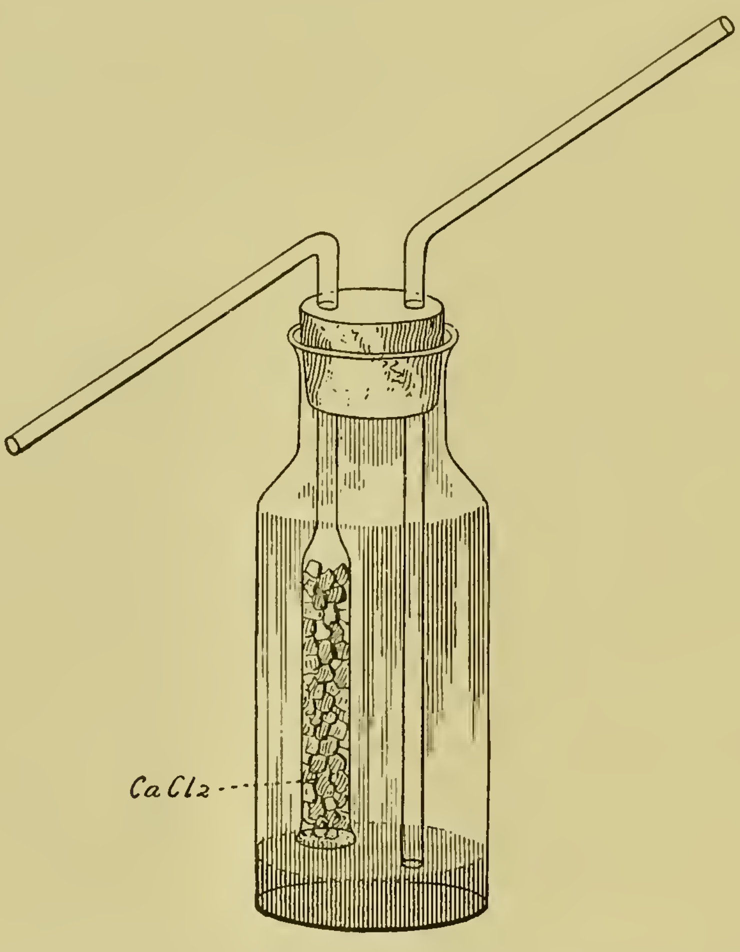

FIG. II.—Ether wash-bottle for blowing ether vapor upon collodion or celloidin sections to fasten them to the slide. The tube of calcium chloride ($CaCl_2$) is for dehydrating the ether vapor.

alcohol, cleared, and mounted in balsam. For clearing a mixture used by Professor Gage for animal tissue will be found to work admirably. It consists of 3 parts of turpentine and 2 of carbolic acid. It clears quickly and will not injure the most delicate tissue. For staining one must use that which seems best adapted for their purpose, but for general study hæmatoxylin seems especially adapted for collodion sections.

Some difficulty may arise in cutting sections that have in them free parts. It sometimes happens that they become detached from the collodion and float away. In this case the section can be collodionized as first suggested by Dr. Mark. This is done by coating the tissue, before each section is cut, with a thin coat of 1 per cent. collodion, using a camel's-hair brush for the purpose; then draw the knife across the tissue very slowly, keeping alcohol dripping o it while the section is being cut. In this way beautiful longisections of large compound pistils can be obtained, in which the sections of ovules, though not held in place by the placenta, will nevertheless remain in their proper position, and perfect serial sections of each ovule obtained. Care should be taken that none of the sections be cut before collodionization; for although it may not always be necessary to keep the parts in place, yet it is always a safeguard against their displacement.

The method given is found to work admirably on very delicate meristematic tissue. No heat being required, the most delicate of tissue will not shrink. Then, too, the shortness of the method commends it to general use. I find that two days or even thirty-six hours is sufficient, and is even better than a longer time, to go through the whole operation of hardening, infiltrating, and sectioning of nearly all kinds of plant tissue. The material may apparently be left in the thick collodion indefinitely without deteriorating. The sections, after being cut, can be handled with a camel's-hair brush without danger of breaking. By a little experience one will find that the method as given will enable him to bring to his hand material with which to pursue with certainty any investigation in histological botany.

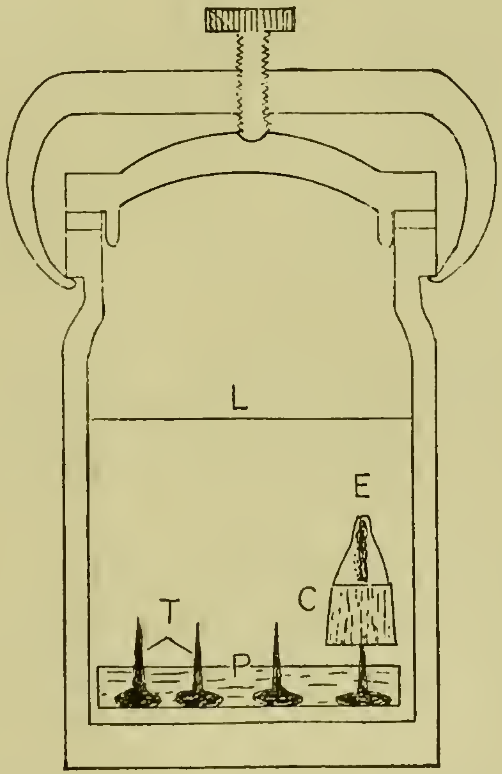

FIG. III.—Jar for Hardening the Collodion of Collodion-imbedded Objects. (From S. H. GAGE.)

P—Plaster of Paris disc in which are imbedded the glass tacks (T).
C—Cork on which the preparation (E) is placed.
L—80 per cent. alcohol.

PREPARATION AND IMBEDDING THE EMBRYO CHICK.

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An excellent method of preparing blastoderms of the chick, of from 1 to 96 hours incubation, both for surface views and for sectioning, is given in Whitman's Methods in Microscopical Anatomy and Embryology (page 166).

With slight modifications, the method is as follows:

1. Break the shell by a sharp rap of the scissors at the broad end; then carefully break away the shell, beginning at the place of fracture and working over the upper third or half.
2. After removing as much of the white as possible without injury to the blastoderm, carefully turn the yolk into a dish of nitric acid (10 per cent.) deep enough to float the yolk, taking care to have the blastoderm on the under side of the yolk.
3. The coagulated white should next be removed from the blastoderm by the aid of a brush or feather, and the egg then allowed to remain in the acid from 20 to 30 minutes.
4. Cut around the blastoderm with sharp-pointed scissors, taking care to cut quickly and steadily. After carrying the incision completely round, float the blastoderm into a watch-glass, keeping it right* side up and flat.
5. Remove the vitilline membrane by the aid of dissecting forceps and the yolk by gently shaking the watch-glass and by occasional use of a needle.† The yolk can sometimes best be washed off by means of a pipette.
6. Wash in water (several times changed).
7. Color deeply with carmine or hæmatoxylin.
8. Remove excess of color by soaking a few minutes in a mixture of water and glycerin in equal parts, to which a few drops (about 1 per cent.) of hydrochloric acid have been added.

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* We find it more convenient to remove the yolk from the blastoderm when it is kept ventral (or wrong) side up.

† We find that a small camel's-hair brush is the best thing with which to remove the yolk.

9. Wash and treat 30 minutes with a mixture of—

Alcohol (70 per cent.) - -	2 parts.
Water - - - - - - - -	1 part.
Glycerin - - - - - - -	1 part.

10. Transfer to pure 70 per cent. alcohol, then to absolute alcohol (95 per cent. alcohol answers every purpose), clarify with creasote or clove oil, and mount in balsam.

For sectioning, blastoderms prepared by this method should be dehydrated, either before or after staining, as is thought best, and immediately transferred to a thin solution of collodion* (2 per cent.), after which they are placed in a thick solution of collodion (5 per cent.) and then arranged for imbedding and sectioning. To accomplish this, the following procedure has been found useful:

With a camel's-hair brush transfer the blastoderm from 95 per cent. alcohol to a paper box. It is better to fill this box partly full of alcohol (95 per cent.) before transferring the blastoderm to it, as the alcohol partially floats the blastoderm and thus facilitates its removal from the brush. As soon as the blastoderm is safely in the box, remove the alcohol with a dropper (do not try to pour it off, otherwise the blastoderm will curl up) and carefully pour in enough thin collodion to cover the blastoderm to the depth of about ½ c. m. The box is now placed in a tightly covered jar to prevent too rapid evaporation and the consequent solidification of the collodion. After the blastoderm has remained a sufficient length of time (from one to three or more hours, depending on the size of the blastoderm) in the thin solution, the collodion is removed with a dropper, and the thick solution poured on. After infiltrating sufficiently with thick collodion, 2 to 10 hours, open the jar and allow a film to form on the surface of the collodion, then fill the paper box with alcohol (60 to 80 per cent.) and allow it to remain till the collodion becomes firm and tough; two to four hours is usually sufficient. Now with a sharp knife a square or rectangular piece of collodion including the blastoderm is cut out and arranged on the cork in any position desired; the block is fastened to the cork, as any ordinary tissue, by simply pouring over it thick collodion, which is hardened by immersing in alcohol (60 to 80 per cent) for from 5 to 15 hours.

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* We have found collodion more satisfactory, on the whole, than celloidin and it is less costly.

To make a 2 per cent. solution, dissolve 2 grams of gun-cotton in 100 c. c. of sulphuric ether and 95 per cent. alcohol, equal parts of each. For a 5 per cent. solution use 5 grams of gun-cotton instead of 2.

For holding the corks under the alcohol the following apparatus has been found more economical and covenient than the method of attaching weights to the corks. The apparatus consists simply of a glass jar, in the bottom of which are fastened several rows of glass tacks. The materials necessary for its construction consist of a wide-mouthed jar, a few pieces of glass rod, and a little plaster of Paris. The tacks are made by heating the glass rod and drawing it out to a rather sharp point. It is then cut off at the right length and the cut end softened by heat and then quickly pressed upon some hard surface, so as to form a sort of head. The tacks are then arranged in rows in some shallow dish, previously oiled, and enough Plaster of paris poured around them to form a layer from 1½ to 2 c. m. deep. When this hardens, the tacks are firmly held in an upright position, and all that remains to be done is to place the plaster disk in the bottom of the glass jar.

To use the apparatus, fill it partly full of alcohol (60 to 80 per cent.). As the specimens are imbedded on the corks, transfer them to this jar, sticking each cork upon a tack.

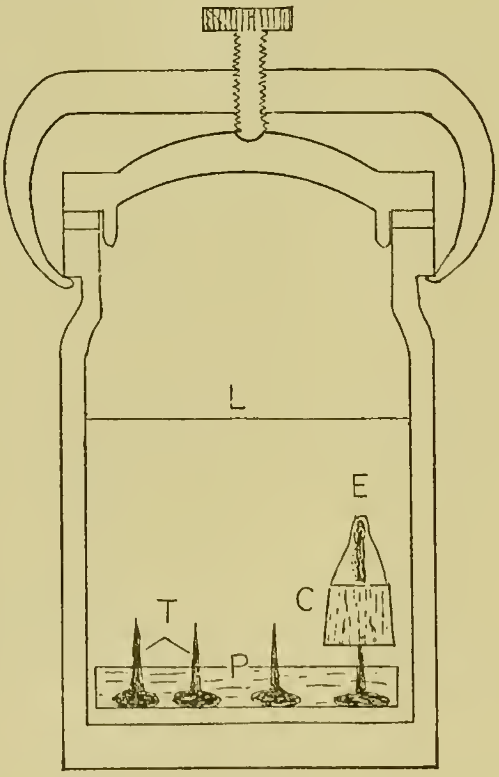

Fig. I.—Jar for Hardening the Collodion of Collodion-imbedded Objects.

(P) Plaster of Paris disc, in which are imbedded the glass tacks. The cork (C), on which the embryo (E) is imbedded, is pushed down upon a glass tack (T), and is held in position under the liquid (L), alcohol or chloroform, while the collodion is hardening. (Devised by Professor Gage; drawn by Mrs. Gage.)

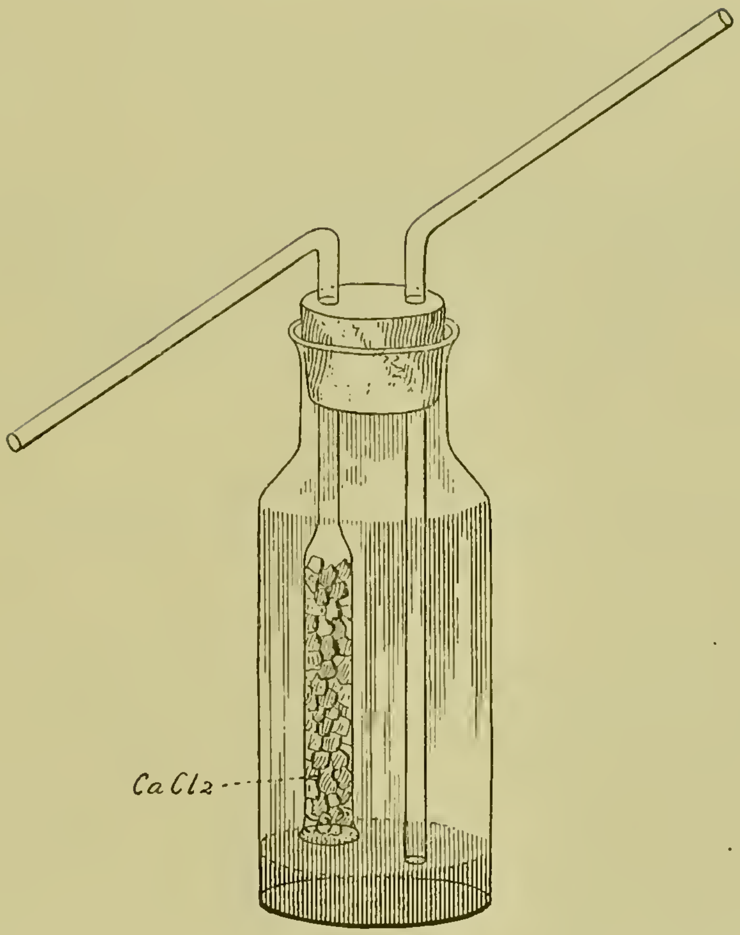

Fig. 11.—Ether wash-bottle for blowing ether vapor upon collodion or celloidin sections to fasten them to the slide. The tube of calcium chloride (CaCl2) is for dehydrating the ether vapor. (From M. B. Thomas.)

THE INTRAMUSCULAR ENDINGS OF FIBERS IN THE SKELETAL MUSCLES OF THE DOMESTIC AND LABORATORY ANIMALS.

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It may be said that muscular fibers in skeletal muscles are of three general forms—the cylindrical, which extend from tendon to tendon, as in the shortest muscles, and in penniform muscles; the conical, with one end at a tendon, the other tapering to a point within the muscle, and the spindleform, with both ends tapering to a point within the muscle. The two latter forms, the conical and spindleform, occur in most long skeletal muscles which are not penniform.

In a previous paper* it was shown that conical and spindleform fibers occur in the longer muscles of minute animals, and the form and relations of the endings were figured. As that paper had a full bibliography, only a few titles will be given with the present paper.

It is the object of the present paper to consider the occurrence of intramuscular ends, and to show their exact form in the skeletal muscles of animals used for dissection and experiment.

The animals examined are carp (Cyprinus carpio), necturus (Necturus maculatus), frog (Rana catesbiana and R. virescens), snapping turtle (Chelydra serpentina), hen (Gallus domesticus), duck (Anas boschas), and English sparrow (Passer domesticus), among the immammalia; opossum (Didelphys virginiana), white rat (Mus ——), mouse (Mus musculus and Hesperomys leucopus), rabbit (Lepus cuniculus), mole shrew (Blarina brevicauda), sheep (Ovis aries), cow (Bos taurus), bat (Vespertilio subulatus, Atalapha cinereus, Atalapha noveboracensis), raccoon (Procyon lotor), kinkajou (Cercoleptes caudivolvulus), dog (Canis familiaris, three varieties, coach, terrier, and spitz), and cat (Felis domestica), among the mammalia.

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* Form, endings, and relations of striated musclar fibers in the muscles of minute animals (mouse, shrew, bat, and English sparrow). Susanna P. Gage. The Microscope, 1888, pp. 225–237 and 257–272, 5 plates, and in abstract in the Proceedings of this Society for 1887.

In each animal as long muscles as possible were chosen, as the *latissimus dorsi*, *biceps femoris*, or *sartorius*, or in the necturus and carp, the respiratory muscles of the throat.

The best practical method for preparing large numbers of slides is that fully described in the proceedings of this Society for 1889, pp. 34–45. In brief, the muscle is dissociated in 20 per cent. nitric acid until the connective tissue is softened, washed in water, dissected in picro-glycerin, and mounted in glycerin jelly, or, after washing with water, the muscle may be kept indefinitely in a saturated solution of alum water containing 2½ per cent. of chloral hydrate. It may then at any time be mounted in glycerin jelly or in Canada balsam.

The careful examination of the intramuscular ends found in this way show that, though there is great diversity of form, several types may be distinguished. There are simple tapering forms, such as are usually described (Fig. 1). These may have a long slender taper of 3 to 7 millimeters, ending in a mere filament, or they may terminate more abruptly in a somewhat brush-like form.

There are also tapering ends which branch; these may be subdivided into four varieties: 1st, the end of the fibers divides into two nearly equal parts, both striated (Fig. 2); 2d, a striated branch is given off at some distance from the tip of the fiber (Fig. 14); 3d, several striated branches are given off at varying distances along the side of the tapering end, the last branch occurring 3 to 8 mm. from the tip of the fiber, at a point where the fiber becomes of full size (Fig. 4); 4th, unstriated projections are given off along the side of the fiber; these may be mere filaments or projections of greater thickness (Figs. 6 and 7).

In the different forms of vertebrates studied there is considerable difference in the occurrence of these types.

In the carp, frog, and necturus the great majority of fibers in the longer muscles extend from tendon to tendon. Even in a sartorius 7 cm. long, taken from a gigantic bullfrog (Rana catesbiana), this is the case. These fibers have a variation in diameter not observed in any of the higher animals when it was certain that the fibers were not tapering to a point. Some of the fibers have only ⅛th to $\frac{1}{10}$th the diameter of others, and without great care one might suppose them to be tapering ends. The frog and necturus, however, undoubtedly have a few fibers which taper to intramuscular ends. The case is much more doubtful in the respiratory muscles of the carp, the longest ones found in that fish. These results differ somewhat from those of other authors. It has been stated that in the frog's *sartorius* the fibers all extend from tendon to tendon. Rollett* says that intramuscular tapering ends are found in the frog and carp. Herzig and Biesiadecki † say that dichotomously divided ends are found in the *gastrocnemius* of frog and the body muscles of *Lota vulgaris.* Further examination is necessary to harmonize or explain these differences.

In the snapping turtle tapering ends are numerous, but no departure from the simple unbranched type was found.

In birds, the hen, duck, and sparrow, numerous tapering ends with a long gradual taper were found. A few of them were divided near the tip into two parts (Fig. 2), and a still smaller number showed branchings of other types (Fig. 3).

In mammals, as in birds, the majority of endings are of the simple, tapering form, but in every species examined there was also a varying proportion of branched endings. In some cases, as the cat, they are rare, except in the diaphragm (Fig. 18); in others, as the mouse, they are comparatively common. The size of the branches varies so greatly that in some cases high magnification (Fig. 19) and great care are necessary for their detection; in others they are patent with a low magnification. In the opossum (Fig. 4) and rodents (Fig. 13) it may be said that the endings have many and large branches, while with the cow (Figs. 9 and 10) and the cat (Fig. 19) the branches are few and small.

As to the particular form of branching in any species, no generalization can at present be made. In most of the species several forms are found more or less like the above types. The figures show a few of the more remarkable endings found in the different species and are selected with the view of showing the diversity of forms occurring.

Other authors have found branched endings in the skeletal muscles of some of the mammals, as Herzig and Beisiadecki, l. c., in the horse; Krause,* in the ocular muscles of the cat; Tergast,† in the ocular muscles of the sheep; Gage,‡ in the cat and rat, and Felix,|| in man and the rabbit.

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*Sitzungsb. d. k. Akad. d. Wissench. Math-naturw. Cl. Wien, Bd. XXXI, 1856, heft I und iii, pp. 176–180, 1 pl.

† Die verschiedenen Formen der quergestreiften Muskelfasern. Sitzungsb. d. k. Akad. d. Wissench. Math-naturw. Cl. Wien, 1858, pp. 146–149, 3 pl.

These statements summarized show that tapering intramuscular ends are common in vertebrates. A few cases are recorded of branched endings in frogs and fishes. These need corroboration. They certainly occur in three widely separated species of birds and in eight of the fifteen orders into which Claus divides the mammals, and in species so widely separated as the opossum and man.

It may safely be predicted that sufficiently careful examination will prove that branched intramuscular ends occur in at least some of the longer muscles of all mammals and birds.

No one factor has yet been discovered which accounts for the usefulness of this branching and its greater development in some species than in others. Neither the comparative activity nor the size of species seems to account for the differences; for the cow and cat have few branches, the opossum and mouse many. Its phylogenetic significance appears to be slight; for, though below birds it is at least rare, it is marked in the lower mammals, while among the higher mammals, the nearly allied cat and dog show a great difference, the cat having few branches, the dog many.

The general occurrence of such branched ends demands an investigation of the mode and time of their development. The appearances in the fully formed fiber and the fact that a nucleus is generally found in the angle formed by a branch lead to the conclusions that a growth of the fiber takes place at the intramuscular end; that the fibrils of which it is composed do not advance at equal pace, and a nucleus formed at the periphery of one fibril may prove the obstacle which turns the adjacent, oncoming fibril from its course, and that thus arises a distinct branch.

A few observations upon a fœtal opossum, a fœtal calf (Fig. 11), and a calf a few weeks old (Fig. 12) give the impression that when the stage is passed when all fibers extend from tendon to tendon,

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* Handbuch der menschlichen Anatomie. Band I, s. 81, Hannover, 1876.

† Ueber das Verhaltniss von Nerven und Muskel, Arch. für mikr. Anat., Bd. IX, 1873, pp. 36–46, 1 pl.

‡ Muscular tissue. Reference Handbook of Medical Sciences, Vol. V, pp. 59–74, illustrated. New York, 1887.

|| Die Länge der Muskelfaser bei dem Menschen und einigen Säugetheiren. Leipzig, 1887, pp. 1–9, 1 Fig. From the "Festschrift für Albert von Kölliker."

fibers are formed which have simple tapering ends; with increase in diameter, some of these become branched.

If this view of the formation of fibers be correct, the sarcolemma can hardly be a preformed sac, but must be a growing deposition either from the fiber itself or the surrounding connective tissue. This agrees with Beale's statement that the sarcolemma is absent in the branching fibers of the tongue, &c.*

The fringed appearance of the extreme end of fibers and their branches, first mentioned in a previous paper (l. c.), seems to be rather more a gradual tapering of the individual fibrils with a loss of striation than a distinctly tendinous structure, although a similar appearance in insect muscle has since been interpreted by van Gehuchten† as a transition to tendon. This appearance was seen in most of the forms examined, but was well marked wherever the tips were comparatively wide, as in opossum (Fig. 4) and rabbit (Fig. 13).

However important these purely morphological points, the physiological side seems still more so. How do these tapering ends join other fibers in order best to execute their work of contraction? From the study of the specimens it seems that no one mode is universal. A tapering end may be closely cemented to a fiber from the same or the opposite direction, and either to a tapering end or a fiber at full size; or, in some cases, it may have a continuation of clear connective tissue which extends on into the general intramuscular connective tissue. Branching ends join other fibers more or less closely, in some cases forming anastomoses with other branching ends, as in the mouse.

In the rat (Fig. 8) and opossum anastomoses were found between two fibers, while in the rabbit, bat, and shrew the indications are strong that fibers unite by the tips of the fringed branches. On these points further investigation is necessary. Such anastomoses occur in the alimentary canal of invertebrates and the iris of birds, while in skeletal muscles Tergast (l. c.), has found them in the ocular muscles of sheep, and Felix says they occur in man.

The close connection of fibers mentioned above should be taken into consideration in any physiological experiments on the transition of a nervous impulse from one fiber to another.

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* On the structure and formation of the sarcolemma of striped muscle, and the exact relations of the nerves and vessels to the contractile tissue of muscle. Trans. Royal Micr. Soc., 1864, pp. 94–108, 2 pl.

† Verhandl. Anat. Gesellschaft, 1889, pp. 100–105.

Summary.

1. In cold-blooded vertebrates many fibers extend from tendon to tendon of the longest muscles, and thus exceed the assumed maximal limit of 4 cm. A few simple tapering intramuscular ends were found in the frog and necturus; none were found in the carp, but many in the turtle.

2. In the longer muscles of the birds and mammals examined most of the fibers end intramuscularly, either by simple tapering ends or by branched ends of varying form. Probably branched ends will be found in the longer muscles of all adult warm-blooded animals.

3. The development of the tapering fibers demands complete investigation, especially with reference to the branched ends. It appears that at first there are no branches, but that they arise by an unequal growth of the fibrils at the tip of a fiber.

4. The branching ends appear to have very close connection with other fibers, in some cases anastomoses occurring. This must have a bearing upon the question of the transmission of a nervous impulse from one fiber to another.

Description of Plate.

Intramuscular ends of fibers from long muscles of the domestic and laboratory animals. The examples were chosen from a great number of drawings to show the diversity of form occurring, though no one can be called typical in any species. The drawings were made with Abbé's camera lucida at a magnification of about 250 diameters.

Fig. 1. A typical, simple, tapering end, such as is found in varying numbers in the necturus and frog and all the birds and mammals examined.

Fig. 2. An ending from a long muscle on the ventral side of the neck of a duck (Anas boschas). The end is dichotomously divided at the tip. This type is found more frequently in birds than in mammals.

Fig. 3. From the *biceps femoris* of a hen (Gallus domesticus), with branches at some distance from the tip. This type is somewhat infrequent in birds.

Fig. 4. From the *trapezius* of an opossum (Didelphys virginiana), with large, strongly marked, spreading branches at the tip and along the side of the fiber to the point where the fiber becomes of full size. This is a remarkably large, well-marked example of a type which is found in greater or less abundance in all the mammals examined, with perhaps the exception of the cow, cf. Figs. 13 and 15.

Fig. 5. From the *sartorius* of the opossum, with several compressed branches. A type not frequent, cf. Fig. 9.

Figs. 6 and 7. From the *biceps femoris* of a white rat (mus), showing unstriated branches. The peculiar fringed appearance in Fig. 7 is quite rare, cf. Fig. 19.

Fig. 8. The anastomosis of two fibers from the *biceps femoris* of the white rat.

Fig. 9. From the *psoas* and Fig. 10 from the *latissimus* of a cow (Bos taurus). Compared with the size of the endings, the branches are small and compressed, while few fibers were found on which branches occur. Endings like Fig. 10 were found in no other animal.

Fig. 11. From the *sartorius* of a fœtal calf about one half meter long. No branched ends were found.

Fig. 12. From the *latissimus* of a calf five or six weeks old. A few simple branching ends were found, the branches being larger in proportion to the end than in the adult. The comparative diameter of the endings in Figs. 11, 12, 9, and 10 is noticeable.

Fig. 13. From the *biceps femoris* of a rabbit (Lepus cuniculus). This type is frequent, though other less branched forms are found.

Fig. 14. From the *latissimus* of the sheep (Ovis aries). Branched ends are frequent.

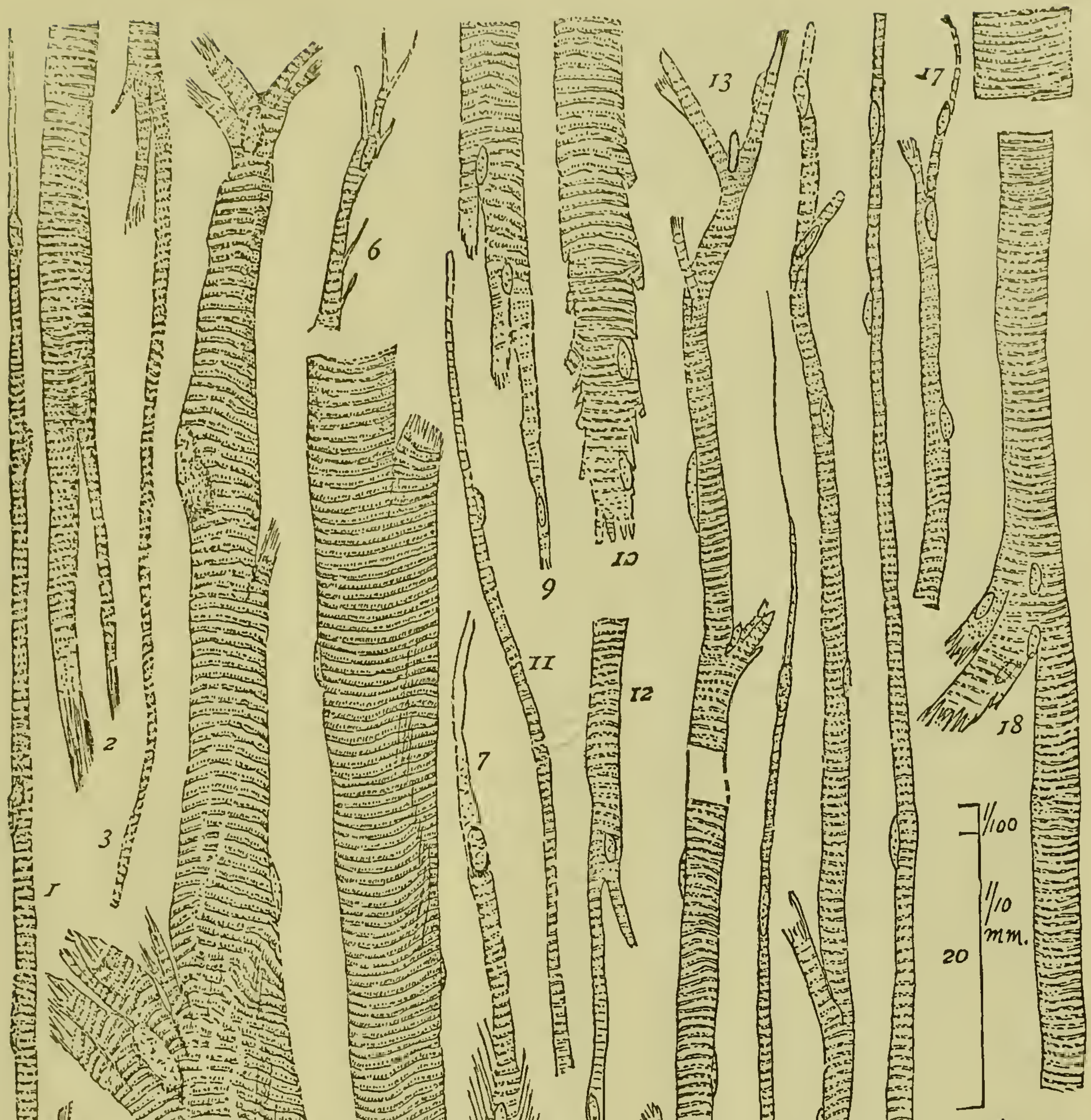
Fig. 15. From the *latissmus* and Fig. 17 from the *sartorius* of a small dog (Canis familiaris) (terrier).

Fig. 16. From the *latissmus* of a large coach dog. This is an unusual form, though branches are frequent.

Fig. 18. From the diaphragm of a half-grown kitten (Felis domestica). Several much-branched forms were found in this muscle.

Fig. 19. From a brachial muscle of the cat (Felis domestica). Striated branches in the ordinary skeletal muscles seem to be rare in the cat.

Fig. 20. Scale of $\frac{1}{100}$ or $\frac{1}{10}$ mm. magnified the same amount as the drawings of the plate.



S. P. Gage ad nat. del.

THE EPITHELIUM OF THE BRAIN CAVITIES.

PIERRE A. FISH, Ithaca, N. Y.

Incidentally during a recent investigation in cerebral anatomy there arose this query: "Is the endymal epithelium ciliated?" Nearly all anatomical authorities agree that it is in the fœtus and early youth, but beyond this age there is a diversity of opinion. The majority believe that cilia do not exist in the adult brain; some that they do exist in the fresh state, and others that they are found only in certain patches of the endyma. Among the last there are those who hold that some of these ciliated patches occur on the plexuses, while their opponents maintain that the epithelium covering the plexuses is pavement-like and devoid of cilia.

In a former research, which included the endyma somewhat indirectly, no cilia were visible. The interest aroused by this apparent discrepancy has been the chief incentive toward prosecuting a more thorough inquiry regarding this important question.

Believing that the morphological differences in these cells in mammals are unimportant, but nevertheless regretting the unavailability of human material, it was thought best under the circumstances to confine the work chiefly to the mesal cavities of the cat's brain, special attention being given to the diacœle, mesocœle, and epicœle. In order to give as general a description as possible of the cells, regardless of the age of the subjects, three different stages were studied: at birth, at six weeks, and the adult. Ciliated cells were found in all which resembled each other so closely in structure that the stages might easily be interchanged. For details of structure high powers should be used; nothing less than 600 diameters will give satisfactory results.

Preparations hardened by Gaules'* method show the cilia quite well, either when sectioned or after being teased.

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* Heat 0.5 per cent. solution of NaCl. to the boiling point and add $HgCl_2$ to saturation. Cool. Put tissues in the solution cold or heated to 40° C. Leave from 10 minutes to 2 hours or longer. Then remove $HgCl_2$ by 70 per cent. alcohol, containing a little tincture of iodine. When the color of the iodine no longer disappears the $HgCl_2$ is removed. Preserve in alcohol.

The epithelium, in some places, may be stripped from the adjacent tissue, as a continuous membrane, and this after remaining a short time in some dissociating agent may be easily teased apart. Scrapings from fresh subjects are preserved almost indefinitely in a saturated aqueous solution of acetate of sodium and may be teased apart in this liquid at any convenient time.

The cells are very short, columnar in outline, and ciliated at their free ends, the long diameter of a cell, including the cilia, being about 9 μ. The cilia are relatively much longer than are usually seen. They represent about one-fourth of the entire length, or 2.5 μ. In hardened preparations they are not always perfectly preserved, often appearing to have been thinned out and broken off. A thorough injection of the hardening agent into the cavities will do much toward obviating this undesirable feature.

The nuclei, more or less oval in outline, nearly fill up the cell-bodies. In none of the nuclei, stained or unstained, could nucleoli be detected, although numerous clear spots could be seen, particularly in the fresh specimens. Luschka (p. 95) believes that the epithelium must have a secretory function, because the clear drops found in the cells resemble those found in the cerebro-spinal fluid. He also states that Kölliker has observed the exit of these drops from the epithelium.

The cell-bodies send off one or more roots into the adjacent tissue, the neuroglia, and by means of them are anchored in their places.

In an examination of a hardened preparation the epithelium of the paraplexus was found to be pavement in character and entirely destitute of cilia. This remarkable transition, from the columnar epithelium of the walls of the cavities to a pavement epithelium of the plexuses, is probably due to the fact that when the blood-vessels of the plexus intrude through the rima into the paracœle (lateral ventricle) the endyma, as an elastic membrane, is pushed along before them, and in accommodating itself to this intrusion the cells, originally columnar and ciliated, have become gradually flattened out and non-ciliated by the pressure. The layer of neuroglia, if present, has for the same reason become infinitesimally thin.

The neuroglia varies in thickness at different places. Under a high power it has a wavy, homogeneous and slightly fibrous appearance, in which are scattered neuroglia cells. The neuroglia passes imperceptibly into the true nervous substance as a connective medium, binding together nerve cells and fibres.

The pleonastic term ependyma* has been made by most anatomists to include both the neuroglia and epithelium. Hirschfeld restricts the use of it to the epithelium ; Quain, to the thin layer of neuroglia between the epithelium and the nervous matter.

The function of cilia is evidently to propel fluids or particles along the cavities which they line. In most instances they line ducts opening externally, and clearly have an excretory function. The adult brain cavities are entirely circumscribed by the endyma in mammals, except in those which are said to possess the metapore (foramen of Magendie). Among these (man at least) it is said that cilia do not exist in the adult stage, as a rule. Except in very rare instances, the human brain is not treated immediately after death and while perfectly fresh, but is allowed to remain uncared for from twelve to twenty-four hours—a delay which may be a more potent factor than is generally imagined in promoting decomposition and thereby obliterating these delicate structures.

The propulsion of the cerebro-spinal fluid through the brain cavities, if cilia are absent, may be accounted for by the brain movements, stimulated by psychical, circulatory, and respiratory influences. In the embryonic stage, where these influences are not yet or only about to be developed, cilia abound.

Summary.

This investigation has demonstrated the existence of ciliated epithelium in the endyma of the adult, as well as earlier stages in the cat. If psychical phenomena are not incompatible with the presence of endymal cilia and if the brain is treated while perfectly fresh, there seems to be no good reason, from a morphological standpoint, why cilia should not likewise exist in the endyma of the human adult.

Bibliography.

1. Gage, S. H. Notes on Histological Methods, 1885–6.

2. Gegenbaur, C. Lehrbuch der Anatomie des Menschen, 1883, pp. 801–2. Believes that the epithelium of the human brain cavities is ciliated in youth.

3. Gray, H. Anatomy, Descriptive and Surgical, 1887, p. 692. "The lateral cavities are lined by a thin diaphanous lining membrane covered by nucleated epithelium (ependyma), with cilia scattered here and there in patches."

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*The more preferable term, *endyma*, is sanctioned by Hyrtl, Wilder, and Spitzka.

4. Heitzmann, C. Microscopical Morphology of the Animal Body, 1883, p. 321.

In this work it is stated that cilia are met with in the investment of the ventricles of the brain and their continuation—the central canal of the spinal cord, and also on the choroid plexuses. Furthermore, that in children their presence is invariable, but in adults they are not found in every case.

5. Henle. Anatomie des Menschen, 1876, p. 367.

The statement is made that ciliated epithelium persists as a rule in the aqueduct, and that the epithelium of the choroid plexus is pavement.

6. Hirschfeld, L. Système Nerveux, 1866, p. 29–30.

Defines ependyma as a single layer of ciliated pavement epithelium, not ciliated on the plexuses.

7. Lee, A. B. The Microtomist's vade mecum, 1890.

A handbook of the methods of microscopic anatomy.

8. Leidy, J. An Elementary Treatise on Human Anatomy, 1889, p. 769.

Uses the term *endothelium*; says that it consists chiefly of short columnar ciliated cells; though the cilia mostly disappear by adult age, except in the ventricular aqueduct.

9. Leydig, Dr. Franz. Traité d'histologie, 1866, p. 201.

Is convinced of ciliary movement in the brain epithelium of young cats. He believes that it also exists in birds, amphibia, and fishes. The cilia are very fine but quite visible.

10. Luschka, Dr. Hubert. Adergeflechte des Gehirnes, 1855, pp. 90, 121, and 169.

The epithelium of the ependyma shows essential differences according to age. In the fœtus and newly-born it is ciliated. In a four-year-old child a mixed epithelium was found in all the ventricles. Normally the epithelium of the plexus of the adult man shows no cilia. Besides man, this author examined epithelium from the brain of the horse, calf, dog, and rabbit, and cilia were found in all.

11. Quain. Elements of Anatomy, 1882, II, pp. 302 and 324.

Defines ependyma as a layer of gelatinous substance (neuroglia) free from nerve cells underneath the lining epithelium.

12. Ranney, A. L. Applied Anatomy of the Nervous System, 1888, p. 308.

"Each lateral ventricle is lined with a delicate layer, called the '*ependyma*,' that is covered with cilia in the fœtus."

13. Satterthwaite, T. E. Manual of Histology, 1881, p. 320.

"The ependyma consists, first, of a finely granular layer covering all the nervous matter bounding the ventricles. Besides the minute granules, this layer contains a few small nuclei here and there, but no fibers. On its free surface rests a single layer of cylindrical epithelium. The cells of this layer have square free ends, while they are anchored by one or more delicate processes which emerge from the attached end and pierce the subjacent granular matrix. These epithelia, in the fresh state, undoubtedly have cilia."

14. Schwalbe. Lehrbuch der Neurologie, 1881, p. 797.

The cilia appear in mammalian embryos, but are absent in the adults.

15. Spitzka, E. C. Article, Histology of Brain. Reference Handbook of the Medical Sciences, vol. VIII, 1889. p. 177.

"On its (endymal) surface it is lined by cylindrical epithelia ciliated at their free ends and rooted in the underlying supporting substance by fine radial fibers. Where the endymal expanse is reflected to cover the various choroid plexuses the epithelium becomes more pavement-like."

16. Wilder, B. G. Article, Gross Anatomy of the Brain. Reference Handbook of the Medical Sciences, vol. VIII, 1889.

Description of Plate.

All of the figures were drawn from the specimens with the aid of Abbe's camera lucida. Some of the details were filled in free-hand.

Zeiss' 8 mm. apochromatic objective was used for figs. 1 and 2. His 2 mm. apochromatic, homogeneous immersion objective, for figs. 3 and 4.

FIG. 1 shows the relation of the epithelial cells to the subjacent neuroglia.

The preparation was hardened by Gaule's method and imbedded in collodion. The section was cut across the mesencephal. *a*, layer of endymal cells showing cilia and nuclei, cilia more or less thinned and broken off. *b*, subjacent layer of neurolgia with nuclei imbedded toward the periphery.

Magnified about 700 diameters.

FIG. 2. Prepared as fig. 1.

The layer of cells is partially detached from the neurolgia, showing their root-like processes.

Magnified about 700 diameters.

FIG. 3. An isolated cell from the diacœle of a fresh specimen. *c*, cilia; *n*, nucleus, showing the clear spots; *r*, root-like process broken off at its end.

Magnified about 2,100 diameters.

FIG. 4. Cells from the epicœle with a portion of the neuroglia (*b*), other letters as in fig. 3.

Magnified about 2,100 diameters.

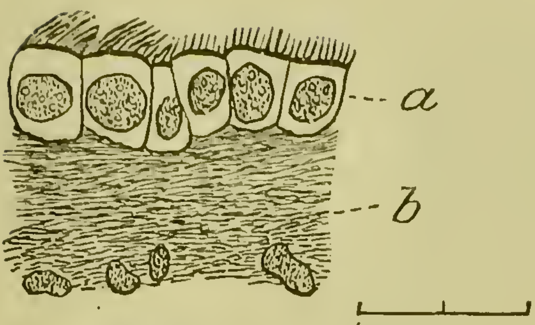

Fig. 1

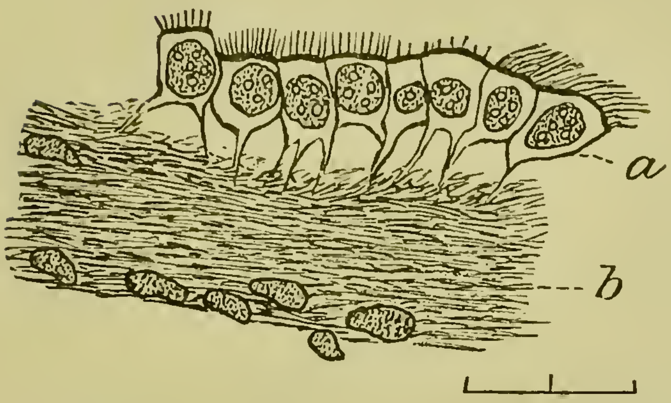

Fig. 2

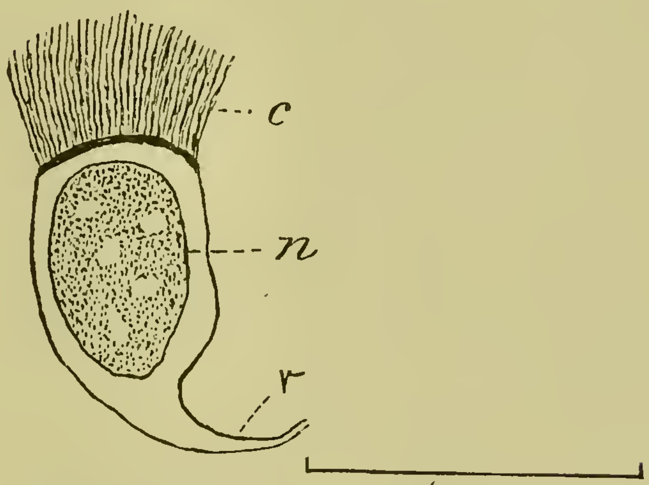

Fig. 3

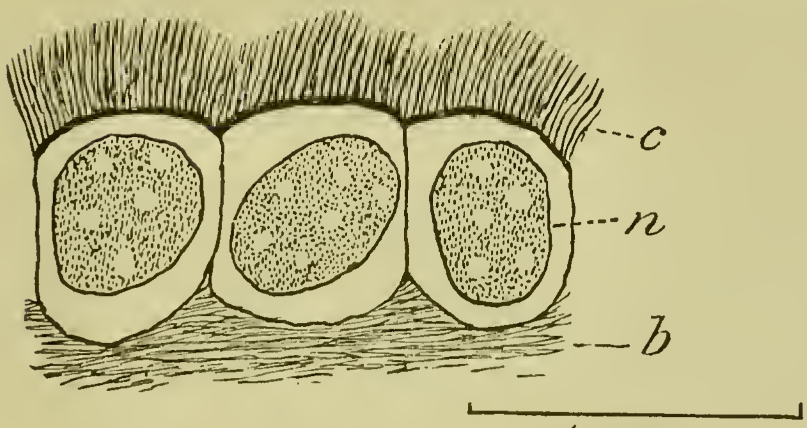

Fig. 4

A COMPARISON OF THE EXTERNAL AND MIDDLE EAR OF MAN AND THE CAT.

THOMAS B. SPENCE, A. B., Cornell, '90.

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Preface.

In the present paper both the macroscopic and microscopic structure of the cat's external and middle ear will be discussed.

The gross anatomy of the cat's ear has already been described a number of times. There are, however, a few points in this comparison which I consider new. These will be specified when the parts are described.

I have found no description of the microscopic structure of the cat's ear. All statements here made concerning it are based on my own investigation.

My thanks are due to Professors B. G. Wilder and S. H. Gage for many valuable suggestions and much helpful criticism.

General Comparison.

It is a well-known fact that the cat's organ of hearing is far more sensitive than man's. Though this superiority may be largely due to the structure of the internal ear, it is fair to suppose that the external and middle ear also play their part in effecting it.

At the first glance this supposition would seem to be warranted. The trumpet-shaped pinna of the cat's ear is certainly a better con-

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* A thesis presented to Cornell University for the degree of A. B., 1890.

ductor of sound than is the nearly flat pinna of man's ear. Its comparatively larger size also seems to be an advantage. If we may follow the teaching of Darwin, it would seem that in the course of evolution man had lost the admirably formed pinna which his ancestors undoubtedly possessed, and that it had become flattened by folding at the helix and antihelix, the point being thus turned in toward the concha. What Darwin considers the remnant of this point is still present in many human ears.

The comparatively larger size of the membrana tympani of the cat's ear, as well as its thinness, probably has some significance. The longer manubrium of the malleus and the slenderness of all the ossicles are also to be noticed.

The two-chambered tympanum of the cat's ear doubtless possesses some advantage over the one-chambered tympanum of man's. The precise effect the second chamber has, may not be clearly understood, but the fact of the presence of the tympanic bulla in all felidæ and in many other animals having an acute sense of hearing cannot be overlooked.

The manner in which the chorda tympani nerve of the cat crosses the tympanum is quite remarkable. The presence of a separate support to raise the nerve clear of the membrana tympani seems positive proof that it is to the cat's advantage to have the nerve thus raised.

The External Ear.

The pinna of the cat's ear, though similar in minute structure to that of man's, presents marked differences from it in form. It is in the shape of a funnel whose top has been cut off obliquely, thus giving it a pointed extremity at one side. Though man's pinna is not pointed, there is a small projection of the cartilage found in many human ears on the caudal edge and near the dorsal part of the helix, which Darwin considers a remnant of the point of the ear. (Descent of Man, vol. I, p, 21.) The concave surface of the cat's pinna is turned latero-cephalad. On the lateral border a pocket is formed as if by the splitting of the cartilage. It is lined with skin and is 1½ to 2 centimeters in length and about 5 millimeters in depth. Just mesad of this pocket is a prominence which is probably homologous with the tragus of the human pinna. Just cephalad of this prominence and separated from it by a notch is a double prominence which may be homologous with the antitragus of man's ear. The dorsal half of this double prominence is covered with stiff hairs. There is no lobe on the cat's pinna, nor are there any folds corresponding to the helix and antihelix of man's pinna. Farther mesad in the concha there are ridges and windings which are difficult of description and which do not appear to have homologues in the human pinna. The most prominent ridge extends cephalo-caudad and terminates at the caudal end in a large rounded prominence. Mesad of this and separated from it by a deep groove is a smaller ridge running parallel to it.

The skin covering the pinna is thick and tough and, except on a part of the concave surface, is covered with hair. On the mesal side of the concave surface there are some long hairs which reach nearly across the pinna. On the convex surface there is a tuft of hair projecting beyond the tip of the ear. Man's pinna has only a few delicate hairs upon it. In both man and cat the skin is connected to the frame-work by areolar tissue, the fibers of which run in all directions. The frame-work is a plate of elastic cartilage. (Fig. 1.)

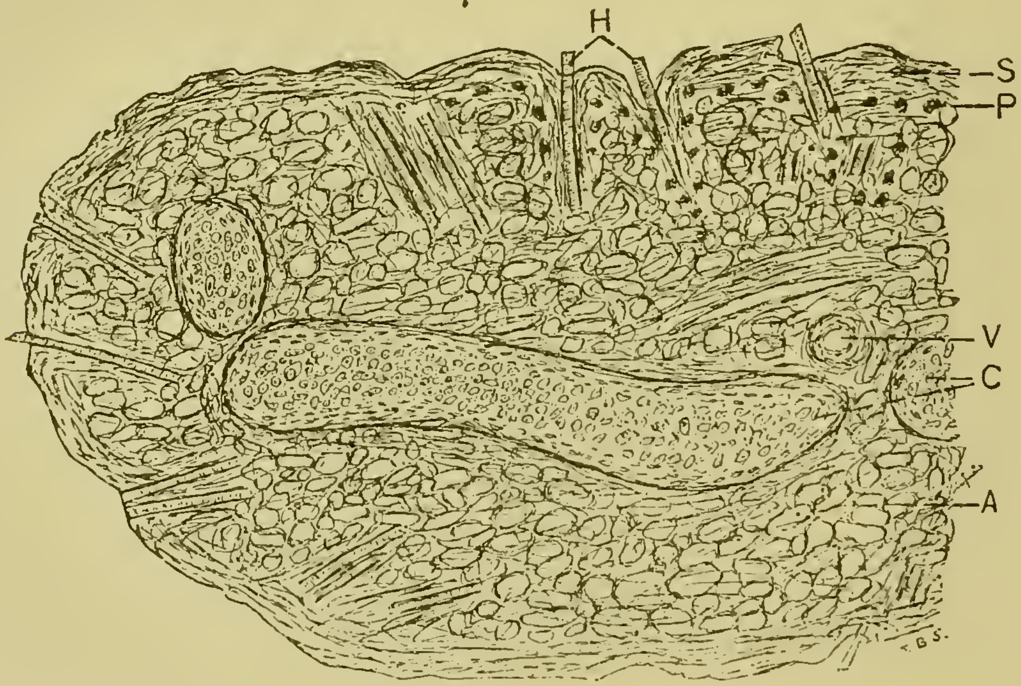

Fig. 1.—Transection of the pinna of a cat. Cartilage cells drawn with a high power.

C—Cartilaginous frame-work.
A—Areolar tissue.
V—Blood-vessel.
P—Pigment.
S—Skin.
H —Hairs.

The external auditory meatus of the cat is 2 to 2½ centimeters in length and extends mesad to the membrana tympani. It is lined throughout with delicate skin. Its frame-work is cartilaginous nearly its whole length, two tubes of cartilage being joined end to end to form the single tube. The dorsal side of the osseous portion of the frame-work is very short, the ventral side being somewhat longer because of the inclination of the membrana tympani. In man the dorsal side of the frame-work is entirely osseous. The ventral side is partly osseous and partly cartilaginous. The meatus of the cat's ear, unlike that of man's, extends into the tympanum for a short distance, thus giving the membrana tympani a position nearly in the middle of the tympanum. The surface of the meatus is uneven because of a number of small prominences upon it.

The structure of the meatus is similar to that of the pinna. There is a frame-work which is partly cartilaginous and a lining of thin skin which is partly covered with hairs. There is little areolar tissue, the space between skin and frame-work being filled with ceruminous glands and fat-cells. The same structures are found in the human meatus (Fig. 2).

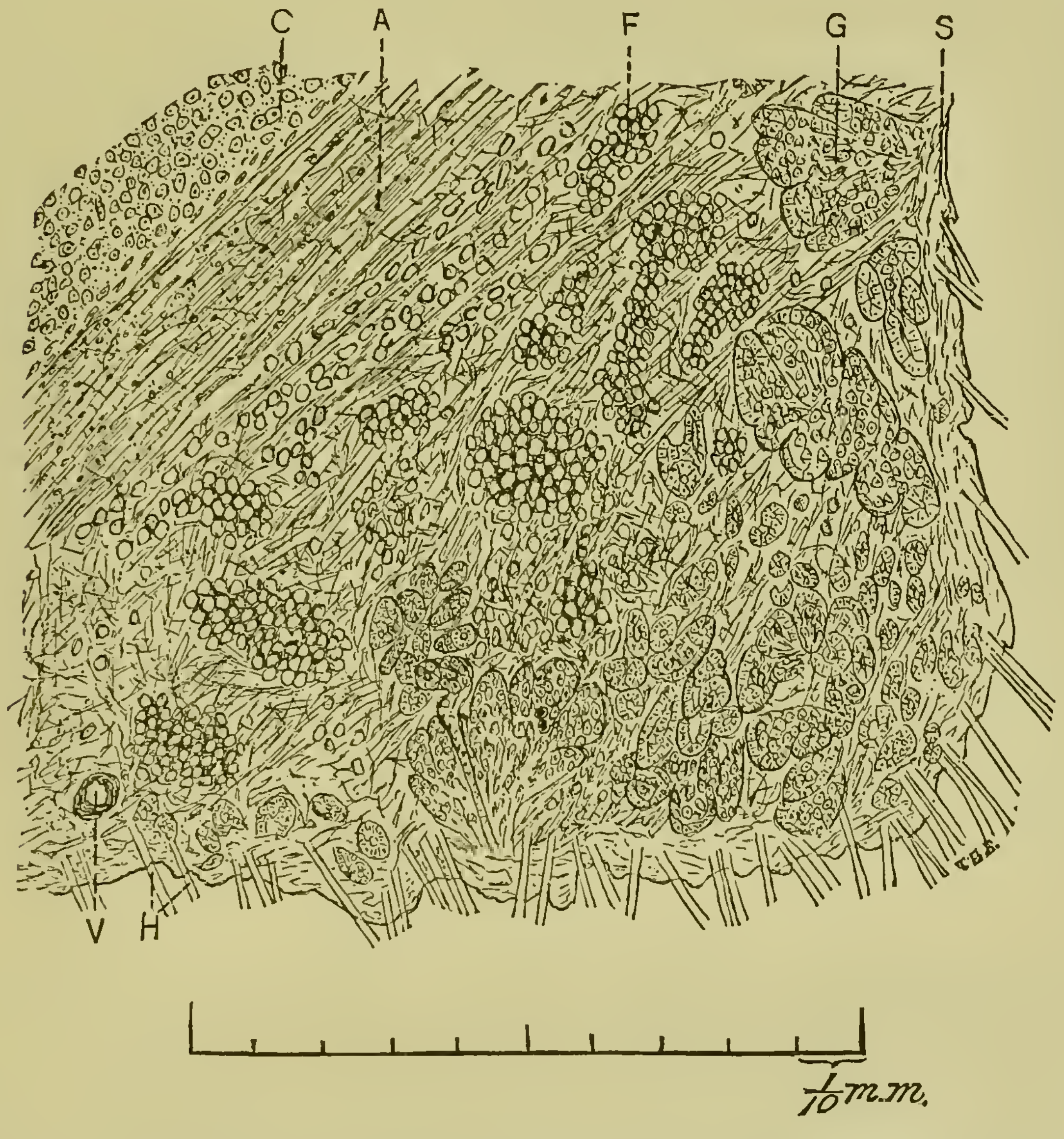

Fig. 2.—Transection of one of the prominences of the external meatus.

C—Cartilaginous frame-work.
A—Connective tissue.
F—Fat-cells.
G—Ceruminous glands.
S—Skin.
V—Blood-vessel.
H—Hairs.

The Membrana Tympani.

The membrana tympani of the cat is attached to a bony ring at the mesal end of the external auditory meatus. It is nearly in the middle of the tympanum, since the meatus extends into the tympanum somewhat. The membrana is more elliptical than in man, and its long axis extends cephalo-meso-ventrad. It is comparatively larger and is more nearly completely traversed by the manubrium of the malleus. It is convex on the mesal side and concave on the lateral, being held in that position by the manubrium of the malleus.

In minute structure the membrana tympani is about the same as in man, but it is thinner and more nearly transparent. Its delicacy of structure probably adds to its sensitiveness. As in man, there is a dermal layer, a fibrous layer, and a mucous layer. The dermal layer is a continuation of the skin of the meatus and is very thin. The fibrous layer is composed of radial fibers on the lateral side and circular fibers on the mesal side. The circular fibers are especially numerous at the periphery. The radial layer is closely connected with the dermal layer, and the circular layer is closely connected to the mucous layer. The mucous layer is covered with nearly spherical, ciliated, epithelial cells (Fig. 3, B.). Brücke states that the epithelial cells of the human membrana tympani are at times ciliated like those through the rest of the tympanic chamber. Hunt says: "The Eustachian tube is an involution of the pharyngeal mucous membrane; the meatus is an involution of the integument; the drum is formed by the Eustachian tube overlapping the extremity of the meatus." The manubrium of the malleus is between the mucosa and the fibrous layer. In the cat the blood-vessels of the membrana tympani form a series of loops from the periphery and also from the manubrium of the malleus, while in man there is a vascular net-work over the whole membrana.

The Tympanum.

The tympanum of the cat varies from that of man in having two chambers, the lateral chamber or tympanum proper, and the mesal chamber, which is larger and takes up the greater part of the large tympanic bulla. These two chambers are present in all felidæ. The tympanic bulla is present in many other animals, but I think there is no animal in which the partition between the two chambers is so nearly complete as in the felidæ. In the cat the partition between the chambers is a thin, long septum, which is incomplete at the dorsal side just ventrad of the fenestra rotunda, leaving a means of communication between the chambers. A partial and incomplete

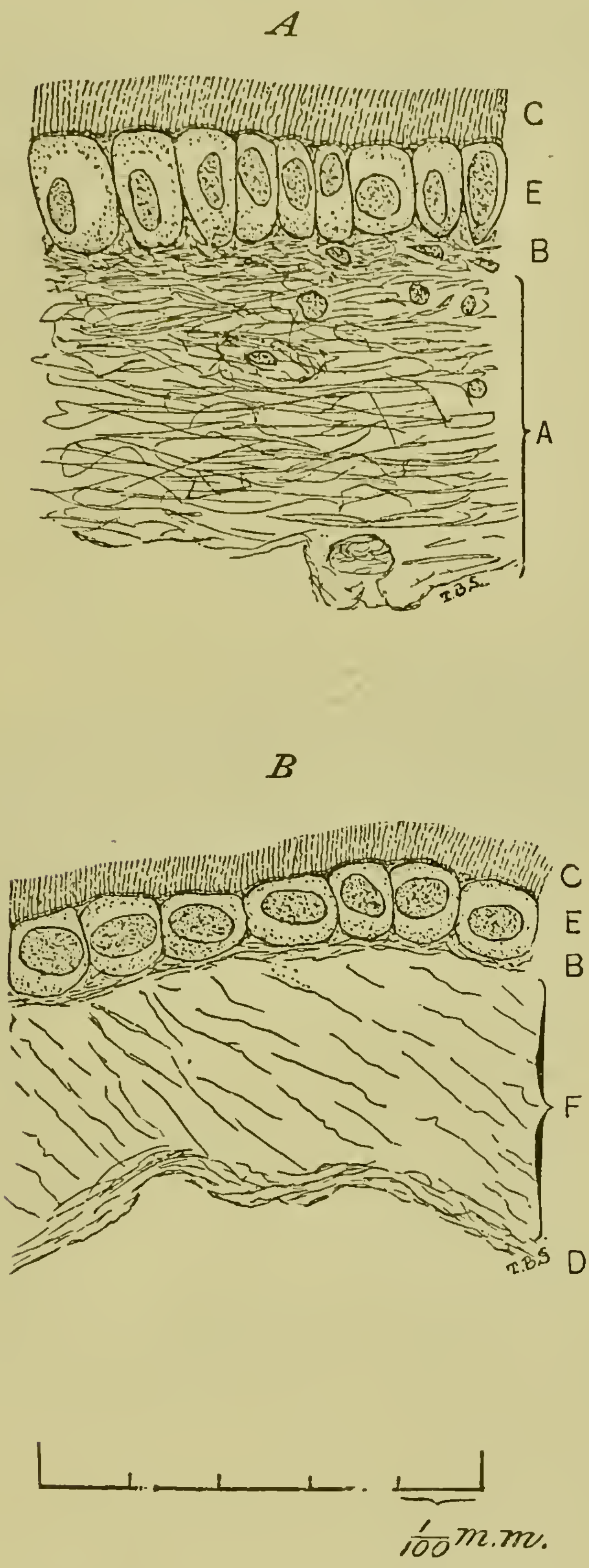

Fig. 3.—A—Lining membrane of the tympanum proper of the cat.

B—Transection of the membrana tympani of the cat. This was so sectioned that it does not show two fibrous layers.

C—Cilia.
E—Epithelial cells.
B—Basement membrane.

A—Connective tissue.
F—Fibrous layer.
D—Dermal layer.

partition is found in the dog's tympanum, running in a plane corresponding to that occupied by the septum in the cat's ear.

The tympanum proper of the cat is bounded nearly the same as in man, and all its walls are covered with a lining membrane. The internal ear is farther dorsad than in man, though this appearance is caused by the relatively larger membrana tympani extending ventrad. The septum between the chambers forms a large part of the mesal boundary. The tympanum, as in man, opens cephalad into the Eustachian tube. The mastoid cells are not so much reticulated as in man. In the orang the reticulations are very noticeable. They are also seen to some extent in the baboon. The tympanum proper contains the ossicles and other structures found in man's tympanum.

The mesal chamber of the cat's tympanum is empty, but it is lined throughout with a thin mucous membrane.

The lining membrane of the cat's tympanum has the same structure

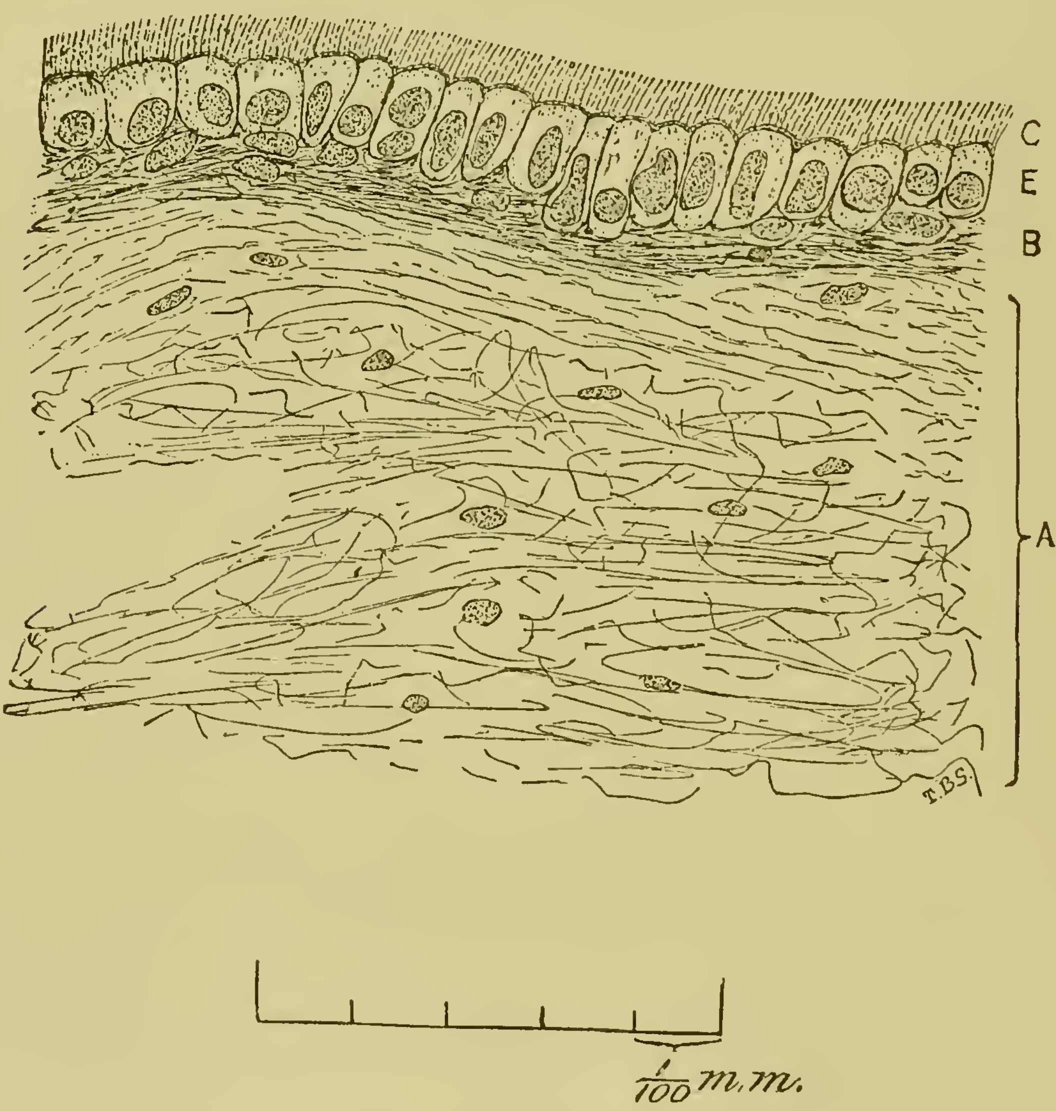

Fig. 4.—Membrane on the bony septum in the mesal chamber of the tympanum of the cat.

C—Cilia.	B—Basement membrane.
E—Epithelial cells.	A—Connective tissue.

as that of man's. It is thin, pale, and slightly vascular and is covered with a layer of ciliated epithelial cells. These cells vary in size in different parts of the chambers. The cilia also vary in length. The largest cells and longest cilia are found in the tympanum proper, both cells and cilia being shorter on the membrana tympani (Fig. 3). In the mesal chamber the epithelial cells on the membrane covering the bony septum are largest (Fig. 4). Those on the dorsal side are nearly as large, being only slightly shorter. They are much larger than those on the ventral side. The membrane, as a whole, is also much thicker, that on the ventral side being remarkable for its extreme thinness (Fig. 5).

As in man, there are three ossicles forming a chain across the tympanum. The head of the malleus and most of the incus are in a fossa in the dorsal part of the tympanum.

The manubrium of the malleus is fastened to the membrana tympani, extending between the mucosa and the fibrous layer. It is longer and more slender than in man. Perhaps its length contributes to its usefulness by conveying slighter vibrations of the membrana tympani than it otherwise would, the ossicle being more easily moved by the long lever. At the junction of the manubrium and neck is the processus brevis, a small process lying in contact with the membrana tympani. On the mesal side there is also a small cylindrical process, to which the tendon of the tensor tympani muscle is attached. This process is wanting on man's malleus. The neck and head of the cat's malleus, extending caudad, form an angle to the manubrium. The processus gracilis is long and flat and arises from the lateral aspect of the neck. The processus gracilis of the malleus of man is long and slender. The head of the cat's malleus is nearly spherical, and articulates by means of a diarthrodial joint with the head of the incus. The processus gracilis is held firmly by means of ligamentous fibers. There is also a suspensory ligament attached to the head, as in man. The tensor tympani muscle is attached to the small process on the mesal side of the neck by means of a short tendon. The muscle is nearly spherical and has its origin in a concavity slightly cephalad of the fenestra ovalis. In man the muscle is long and lies in a canal extending dorsad of the Eustachian tube and parallel to it.

The incus is of nearly the same shape as in man, resembling a molar tooth with two divergent fangs. The head articulates with the head of the malleus; the long process articulates with the head of the stapes by means of the lenticular process and a diarthrodial

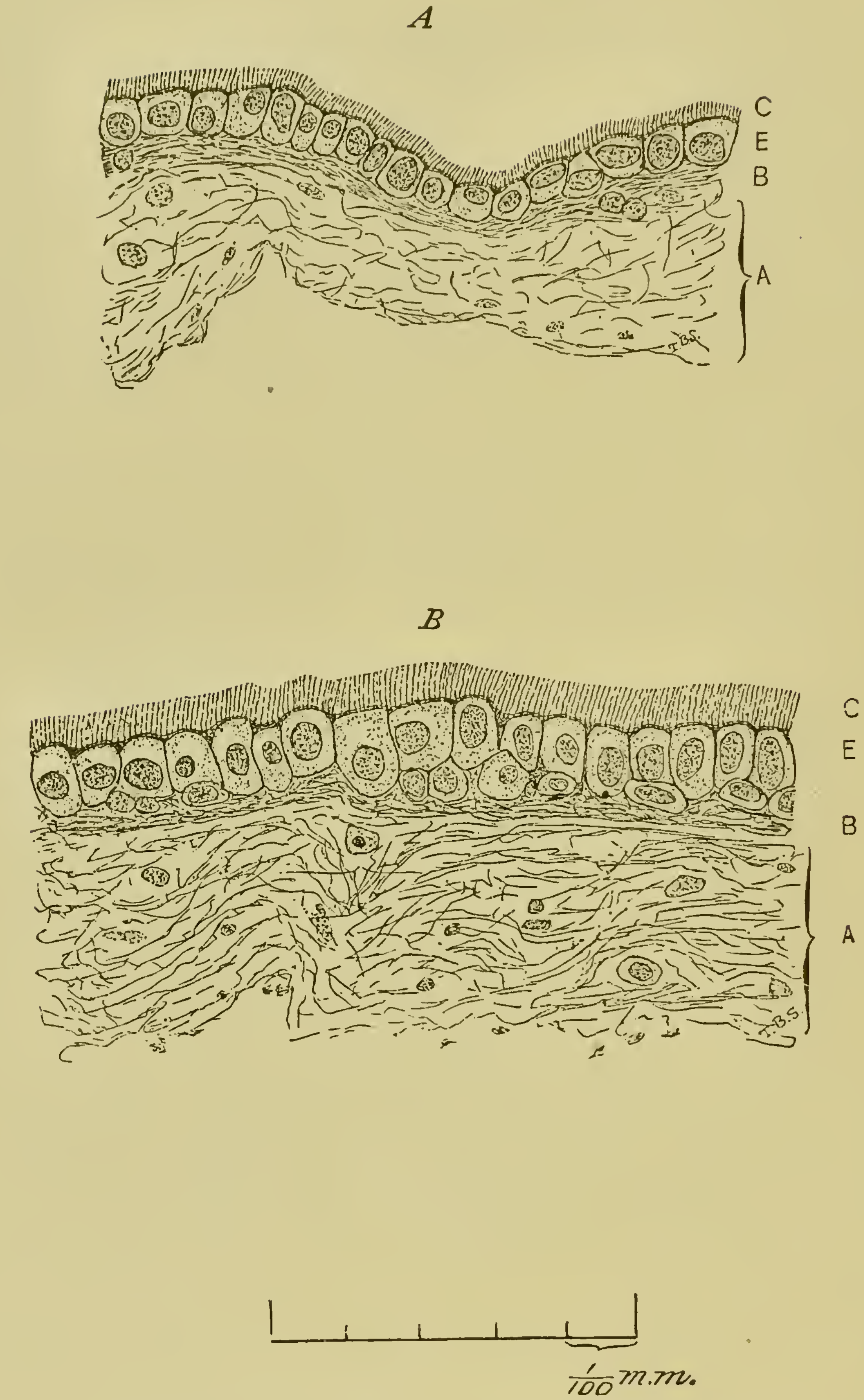

Fig. 5.—A—Membrane from ventral side of the mesal chamber of the tympanum of the cat.

B—Membrane on dorsal side of the mesal chamber of the tympanum of the cat.

C—Cilia.

E—Epthelial cells.

B—Basement membrane.

A—Connective tissue.

joint, and the short process is held by a ligament at the caudal part of the fossa, which is occupied by it and the heads of the incus and malleus. Straus-Durckheim describes the lenticular process as a separate bone. I think it is separate only in early life, as in man. "In early life the lenticular process is a separate ossicle." (Allen's Human Anatomy.)

The stapes of the cat is slight and fragile. As in man, it consists of a head, neck, two crura, and a base. The crura do not spread as widely as they do in man, joining the neck at a sharper angle. They are comparatively longer than in man and are very slender. The base is formed by a slight, elliptical ring of bone and a very thin plate of bone extending across the ring. The base is attached to the edge of the fenestra ovalis by ligamentous fibers, as in man. In this way the fenestra is completely closed. The tendon of the stapedius muscle is inserted upon a bony process on the caudal part of the neck of the stapes. The muscle is very small, and originates in a depression which is separated by a bony septum from the fossa occupied by the incus.

The chorda tympani nerve, as in man, extends across the tympanum, running dorso-cephalad. It crosses the malleus nearer the manubrium than in man, being entirely ventrad of the stapes and

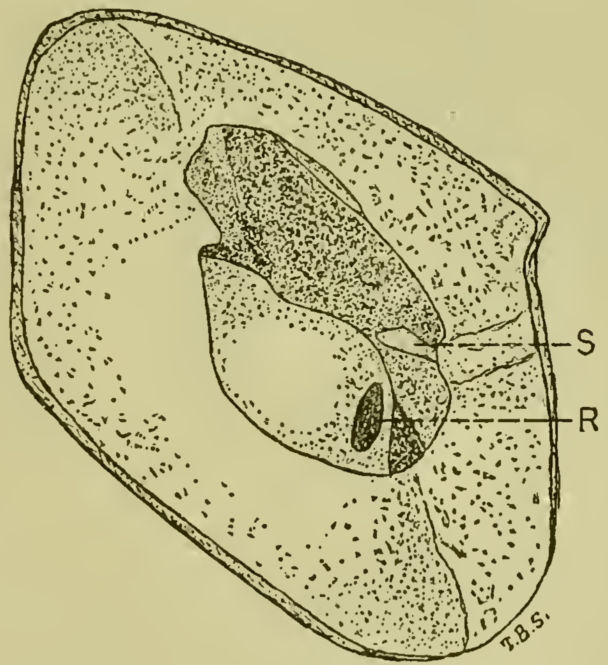

Fig. 6.—The left tympanum of the dried skull of a leopard. The bulla was broken into on the ventral side. Twice the natural size.

R—Fenestra rotunda. S—Support of chorda tympani nerve.

incus. This relation to these bones is in part due to the fact that the process of the incus does not extend far into the tympanum. The position and relations of the chorda also differ from those in man, in that the nerve is raised clear of the membrana tympani until it reaches the manubrium of the malleus. This is effected by means of a small support, which in early life is cartilaginous, but later becomes calcified. This support is 2 to 3 millimeters in length, about a millimeter in thickness at the base, and tapers gradually from base to point. It is joined by ligamentous fibers to the tympanic ring and extends into the tympanum cephalo-mesad (Figs. 6 and 7).

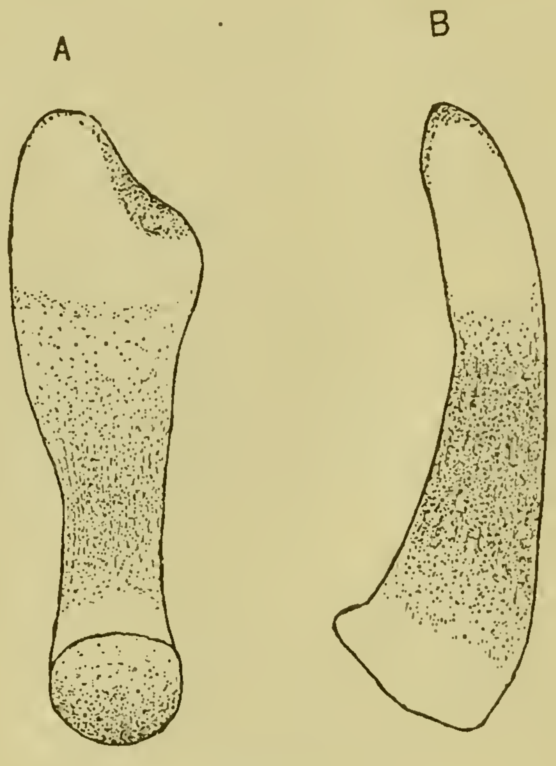

Fig. 7.—The support of the right chorda tympana nerve of the leopard.

A—Dorsal aspect. B—Cephalic aspect.

Magnified about 15 diameters.

The nerve enters the tympanum at the base of the support and runs along its lateral side to its free end, whence it stretches across to the manubrium of the malleus. This support, like the ossicles of the tympanum, is infolded by the lining membrane of the tympanum (Fig. 8). The membrane, however, does not adhere to the whole length of the support, but spreads out at the base somewhat in the manner of the canvas of a tent upon its pole. The fold of membrane extends beyond the end of the support and envelops the nerve until it reaches the manubrium. There is no fold of mem-

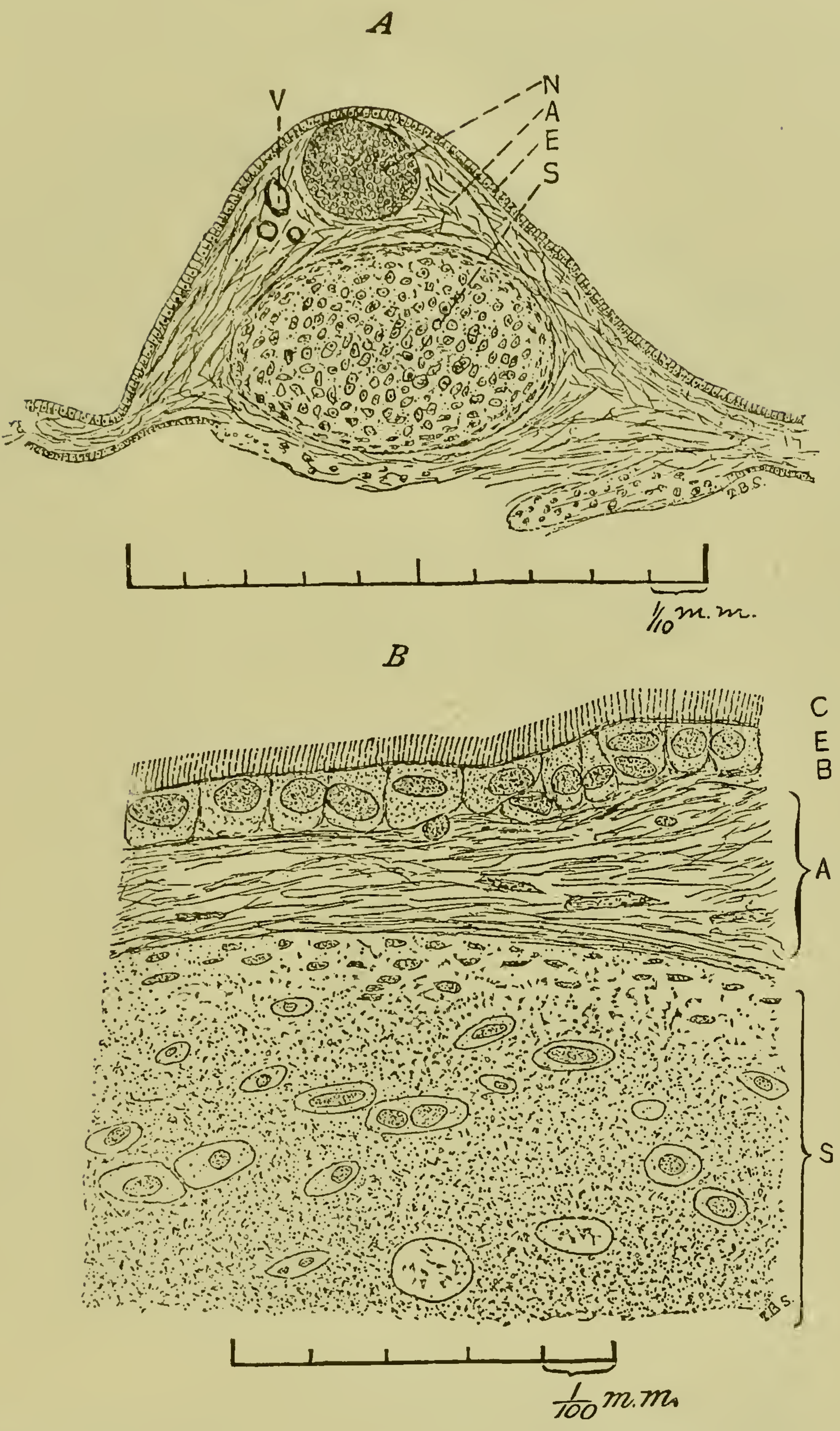

Fig. 8.—A—Transection of the chorda tympani and its support. The details of the nerve and cartilage were filled in with a high power.

B—The support of the chorda tympani, and the infolding membrane.

N—Chorda tympani.
A—Connective tissue.
E—Epithelial cells.
S—Support of chorda tympani.
V—Blood-vessel.
C—Cilia.
B—Basement membrane.

brane between the nerve and the membrana tympani, there being a clear space between them (Figs. 9 and 10).

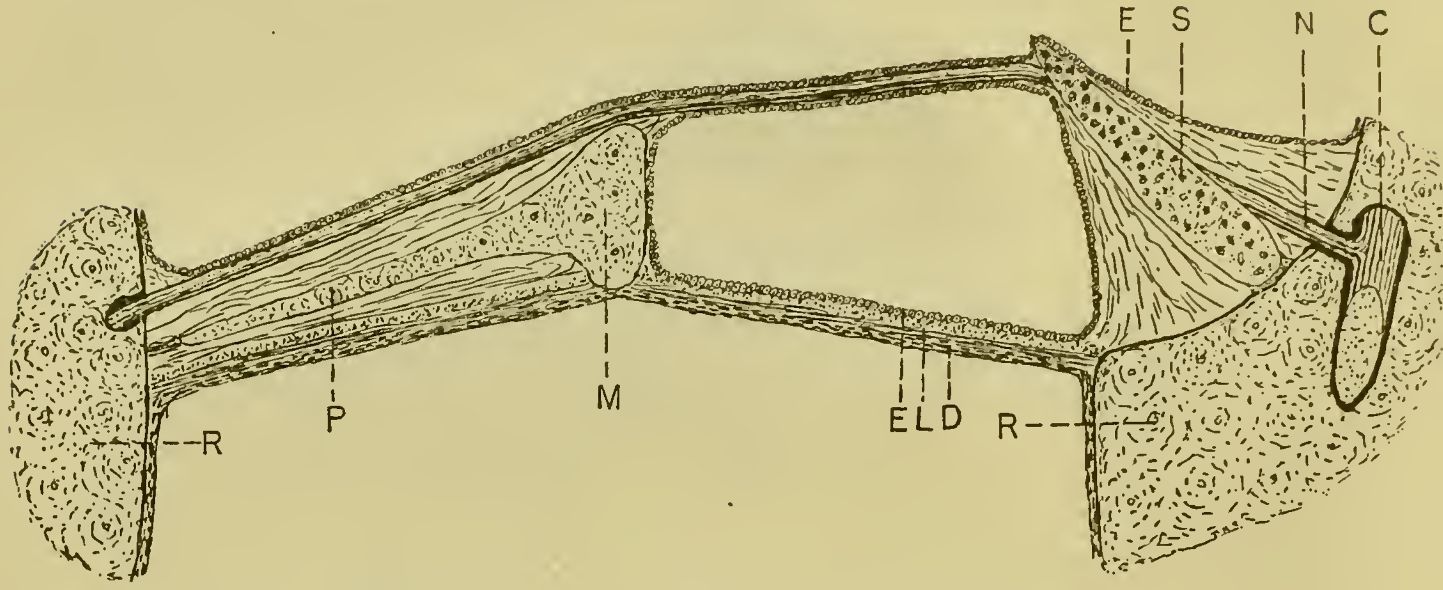

Fig. 9.—Diagram of a section of the membrana tympani of the cat, to show the relations of the chorda tympani and its support.

C—Seventh nerve.
N—Chorda tympani.
S—Chorda tympani support.
R—Tympanic ring.
E—Epithelial layer.
L—Fibrous layer.
D—Dermal layer.
M—Manubrium of malleus.
P—Processus brevis.

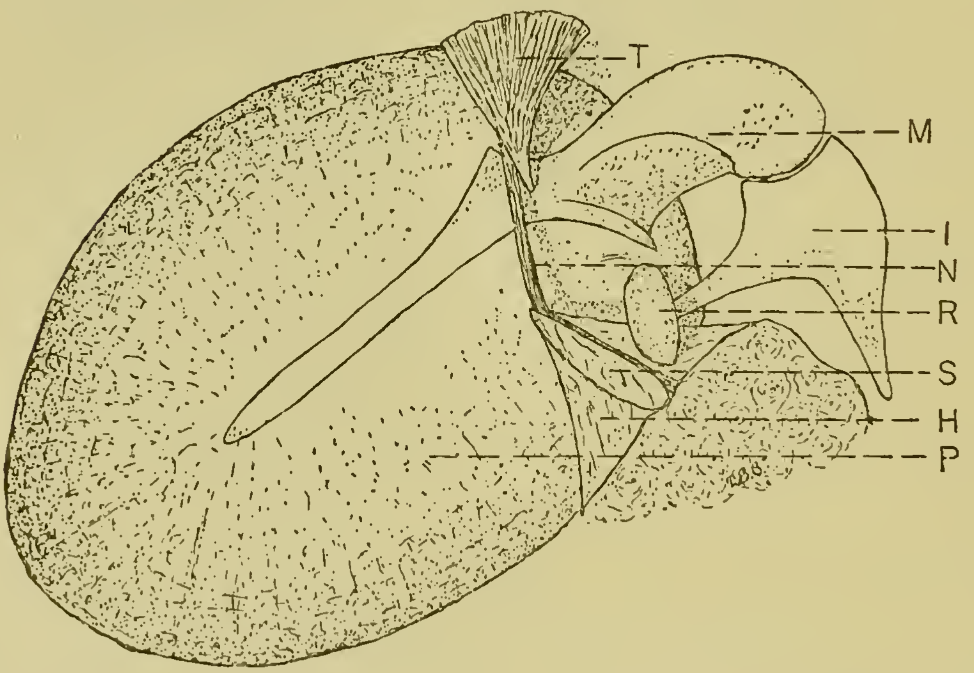

Fig. 10.—Diagram showing the relations of the chorda tympani and its support in the right tympanum of the cat.

T—Tensor tympani.
M—Malleus.
I—Incus.
N—Chorda tympani.
R—Stapes.
S—Support of chorda tympani.
H—Infolding membrane.
P—Membrana tympani.

After much careful study of specimens and literature I failed to find a suggestion of the presence of such a support in man. Neither have I in all the literature found mention of it in the cat or any other animal. On dissection, however, I find a similar support of the chorda tympani nerve is present in the lynx and leopard and think it may be a characteristic of the felidæ. It was still present in the dried skull of the leopard and also of the cat. I examined the dried skulls of a few other members of the group and did not find it; but I also failed to find it in one dried skull of the leopard, in two of the lynx, and in a number of the cat. It may be that the animals examined were not old enough for the cartilage to have become calcified, or that calcified cartilage was removed in cleaning the skulls. Heads with the soft parts still in place must be dissected in order to be sure that the support is not present.

I found a support similar to that found in the cat in a dried skull of the hyena.

On dissecting the ears of an orang, baboon, dog, sheep, cheiromys, and loris I found that the support of the nerve was absent in all except possibly the sheep.

In the sheep's tympanum is a small cartilaginous process in a position corresponding to that occupied by the support of the chorda tympani of the cat. It is infolded in the lining membrane, but I am in doubt whether or not it acts as a support to the chorda tympani. The nerve does not come in contact with the cartilage, but it may be slightly supported by the fold of membrane which passes over the top of the cartilage.

It would be interesting to know the object of this raising of the chorda tympani. Perhaps it is to prevent irritation of the nerve by the vibrations of the membrana tympani. Perhaps—and this hypothesis seems to me the more probable—it is to prevent an interference with the vibrations of the membrana tympani.

The Eustachian Tube.

The Eustachian tube extends from the pharynx dorso-latero-caudad to the tympanum. It opens into the tympanum on the cephalic side just dorsad of a projecting shelf of bone. It is about a centimeter and a half in length and opens into the pharynx through a horizontal slit, which is farther cephalad than the corresponding opening in the pharynx of man.

The frame-work of the tube is formed almost entirely of cartilage and fibrous tissue, being bony only where it penetrates the wall of the tympanum. The frame-work of man's Eustachian tube is osseous for about one-third of its length from the tympanum. The cartilage of the cat's tube consists of a long strip curled upon itself in the form of a trough, a transection being crescent-shaped, and extends the whole length of the tube. It does not extend along two sides of the tube, as in man, but only curls around what is known as the safety tube. It loses its trough-shape near the ends, flattening out somewhat. Glands are found opening into the tube along its whole length, but they are most numerous near the pharynx. The lining membrane of the Eustachian tube, unlike that of the tympanum, is

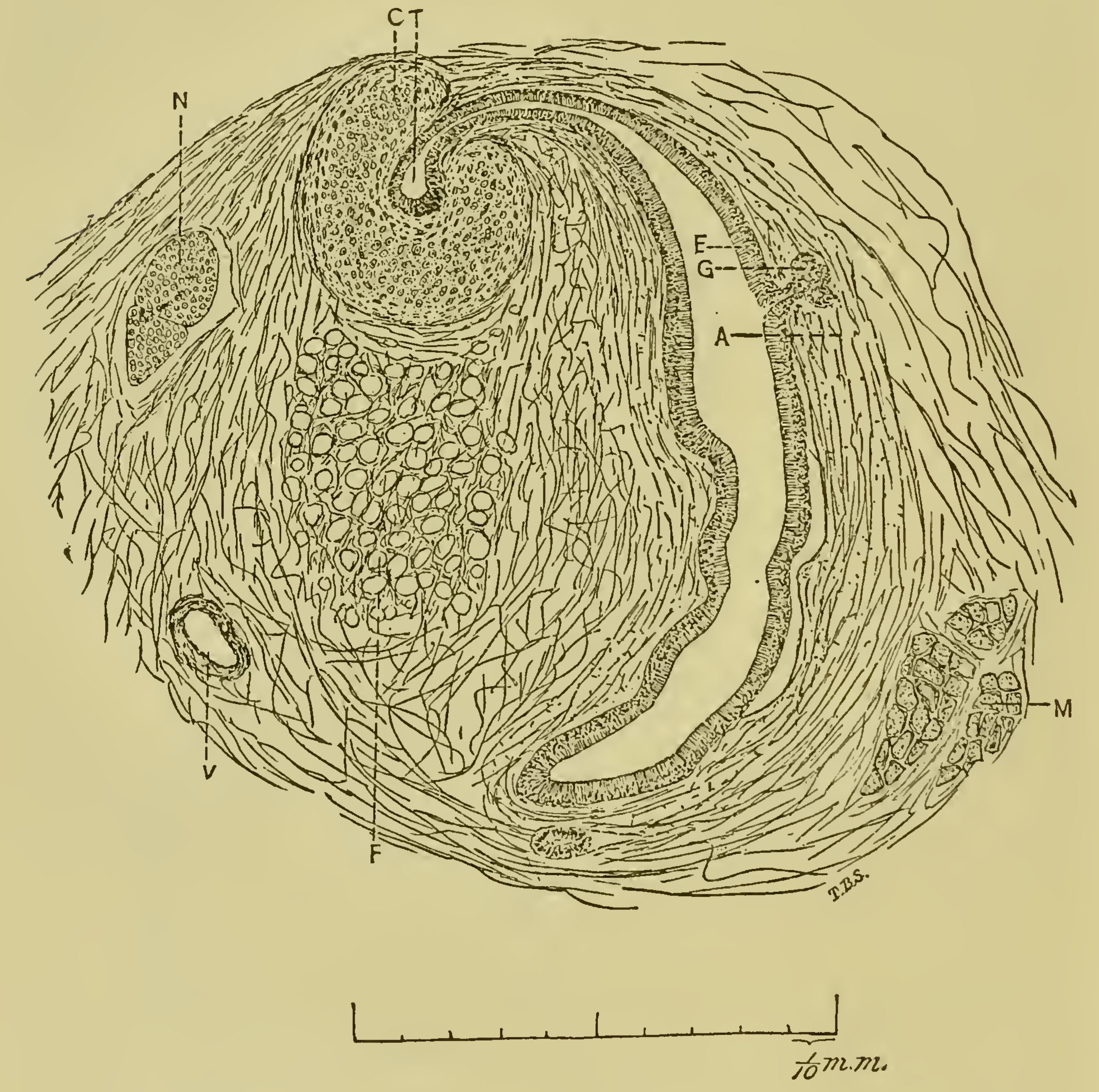

Fig. 11.—Transection of the cat's Eustachian tube.

T—Safety tube.
C—Cartilage.
E—Epithelium.
G—Gland.
A—Connective tissue.
N—Nerve.
V—Blood-vessel.
F—Fat-cells..
M—Muscle fibers.

covered with three layers of epithelial cells. The ectal layer is ciliated throughout and is made up of columnar cells. The ental layers of cells are smaller and more irregular than the ectal layer (Figs. 11 and 12).

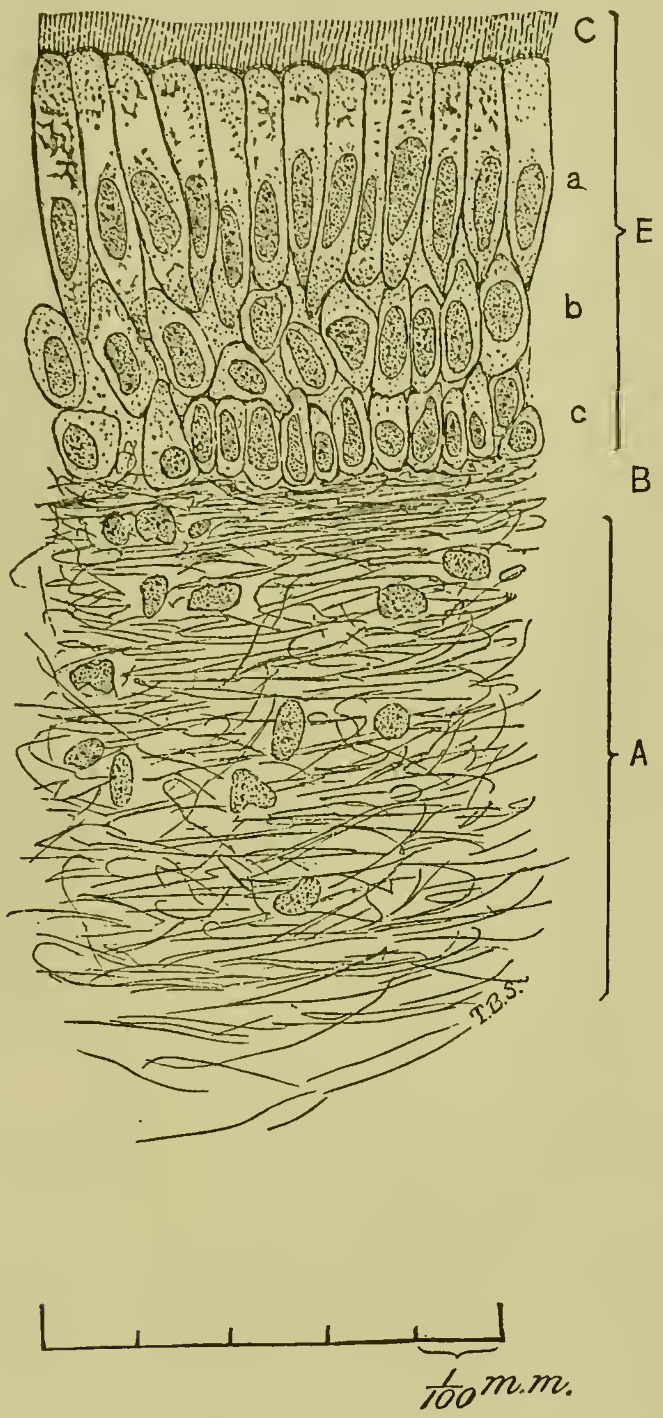

Fig. 12.—Lining membrane of the Eustachian tube of the cat.

C—Cilia.

E—Epithelium; *a*, *b*, and *c*—ectal, middle, and ental layers of epithelial cells.

B—Basement membrane.

A—Connective tissue.

Summary.

The pinna of the cat is pointed, while that of man is folded over at the helix. The helix and antihelix are not present in the cat's pinna. There are, however, ridges and grooves not found in man's.

The external auditory meatus of the cat has a frame-work which is cartilaginous for nearly its whole length. The frame-work of man's external meatus is almost entirely osseous.

The cat's membrana tympani is thinner than man's.

The cat's tympanum has two chambers, while man's has but one.

The ossicles of the cat's ear are more slender than the corresponding human ossicles.

The tensor tympani of the cat is nearly spherical, while that of man is long and slender.

The chorda tympani of the cat is raised clear of the membrana tympani by means of a small support. This support has not hitherto been described nor even mentioned.

The chorda tympani support is also present in lynx, leopard, and hyena. What is probably the homologue of this support is found in the sheep.

The frame-work of the cat's Eustachian tube has cartilage for nearly its whole length; that of man's tube is bony for nearly one-third of its length from the tympanum. The cartilage does not extend so far around the tube in the cat as in man.

The minute structure of all parts of the external and middle ear is very similar in man and the cat.

Methods.

DISSECTING.

Most of the dissections were made from the meson laterad. The head is first medisected, and then the internal ear is nipped away. The bone of the internal ear is brittle and hard, and since the stapes is closely attached to it by ligamentous fibers at the fenestra ovalis, much care is needed lest the stapes be pulled away with a piece of the bone. This is especially true of the cat, in which the stapes is very slight. By this mode of dissection the relations of the tympanum, membrana tympani, and Eustachian tube can be clearly shown. The relations of the ossicles are also easily seen since these bones are shown *in situ*. The chorda tympani and its support can likewise be shown to advantage. The support can also be shown by breaking into the bulla and through the bony septum from the ventral side, the internal ear being left in position.

DRAWING.

The drawings of the microscopic sections were all made by means of the Abbé camera lucida. They were made with a pencil, and then transferred in ink.

HARDENING, IMBEDDING, ETC.

Picric alcohol gave the best results as a hardening agent. Flemming's chrom-acetic acid is not good when cilia are to be shown.

The tissues were imbedded in paraffin when either of the above re-agents was used.

The membrana tympani was hardened *in situ* in a .001 per cent. solution of palladium chloride. The manubrium and tympanic ring were decalcified by means of a .1 per cent. solution of hydrochloric acid. The membrana tympani in this case was imbedded in collodion. The epithelial layer did not show clearly by this method, but the failure may have been due to my inexperience in the use of the re-agents and imbedding material.

Of the stains used hæmatoxylin and fluorescine gave the best results. Ammonia carmine and alum carmine, with or without fluorescine, both failed to bring out the parts with distinctness.

Xylol balsam and chloroform balsam were used indifferently as mounting media.

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STRUCTURE OF THE STOMACH OF AMIA CALVA.

GRANT S. HOPKINS, B. S., Cornell, '89, Ithaca, N. Y.

The stomach of *Amia calva** presents the same general appearance as is seen in other members of this or of higher groups, but a close examination reveals a condition found in but few fishes and in none of the higher animals.

The muscular fibers of the stomach are arranged, as is usual, into three coats—the longitudinal, circular, and oblique. The ectal longitudinal coat is extremely thin, and in places does not form a continuous layer. The inner coat is incomplete, being confined mainly to the cardiac end of the stomach. As the œsophagus enters the stomach the transversely striated muscular fibres are gradually replaced by smooth muscular fibers, the latter kind alone being found in the pyloric half of the stomach and in the intestine.

It is, however, to the mucous membrane of the stomach that special attention will be given. The most interesting feature noticed here is the existence of a ciliated epithelium. So far as I know, this ciliated character of the stomach of the Amia was first discovered by Prof. S. H. Gage in 1886.

The ciliated epithelium extends uninterruptedly from the beginning of the œsophagus to within about one centimeter of the pylorus.

The direction of the ciliary currents is from the cardiac towards the pyloric end of the stomach.

In order to determine whether this ciliated stomach was really a digestive organ or not the following experiment was tried: A large tadpole was given to the fish; two and one-half hours afterwards the fish was killed and the contents of the stomach examined. The tadpole was found partially digested, small pieces of muscle, nerve, etc., being found in the stomach in great abundance, showing conclusively its digestive character.

When empty, the mucous membrane of the stomach is thrown into longitudinal folds or rugæ, and it also has that soft, velvety appearance characteristic of the lining membrane of this organ.

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* Aganoid fish, the dog fish or lawyer.

The cells of the surface epithelium are extremely long and slender. Each cell contains a rather large, oval nucleus situated at a variable distance from the free end. This variation of level in the situation of the nuclei often gives the epithelium the appearance of being stratified. The attached ends of the cells are continued into long thread-like processes, upon some of which are visible slight enlargements. These thread-like processes of the cells do not stop at what appears to be the basal surface of the epithelium, but they extend down into the mucosa.

Ludwig Edinger says that a ciliated epithelium never exists in the stomach of fishes. Trinkler, however, found ciliated cells in great numbers in the stomach of two bony fishes, the perch and the pike.

As to the glandular character of the stomach of the Amia, I find the two common kinds of glands, the so-called cardiac and pyloric glands, the former being the more numerous. The mouths of the cardiac glands are lined by ciliated epithelium, the deeper portion by granular cells of a more nearly cubical or spherical form.

The depth to which the ciliated epithelium extends into the mouth of a tubule compared to the whole length of the tubule is as 1 to 4.5, very nearly. These glands extend to within about 1.5 c. m. of the pylorus, where they are replaced by the pyloric glands. The latter are lined throughout their whole extent by columnar cells, having the same appearance as those of the surface, but somewhat shorter. In many cases two or more glands open into a single mouth. For a distance of about ½ c. m. caudad of the point where the pyloric glands first appear they are lined by ciliated epithelium as in the cardiac glands. To what depth the cilia extend into the mouths of the glands possessing them could not be determined from the specimens at hand, but by further examination I hope to settle this point. The greater number of these glands, however, have no cilia.

Methods.

The methods here given are those used in the anatomical laboratory of Cornell University.

NITRIC ACID METHOD.

For demonstrating the muscular layers of the stomach it is placed in nitric acid (20 per cent.) for a short time. No definite length of time for leaving it in the acid can be given, as it varies in different cases, due chiefly to variations of temperature. When the connective tissue is sufficiently softened the muscular coats can be separated from each other very satisfactorily.

This is a most excellent method also for obtaining isolated gastric tubules. After macerating the stomach in the acid for a sufficient length of time it is washed in water and then placed in a saturated aqueous solution of alum, where it may be kept indefinitely. To isolate the tubules a small portion of the mucous membrane is teased on a slide with needles, stained, and mounted in glycerin jelly.

PICRIC ALCOHOL METHOD.

For isolating epithelial cells this method serves most admirably. The tissue is placed in 25 per cent. alcohol, to which a little picric acid has been added (95 per cent. alcohol, 25 c. c.; water, 75 c. c.; picric acid, $\frac{1}{10}$ gram) for a few hours, or until the cells can be easily isolated. The epithelium must not be left in the picric alcohol too long, otherwise the cells will deteriorate. Preserve in glycerin or glycerin jelly.

MERCURIC CHLORIDE METHOD.

I prefer this method to all others for hardening the stomach for sectioning. The stomach is fastened to cork, with pins previously coated with vaseline to prevent their being corroded by the mercuric chloride, and immersed in a saturated aqueous solution of mercuric chloride plus ½ per cent. sodium chloride ($NaCl$, ½ gram; $HgCl_2$, 5 grams; H_2O, 100 c. c.) for ½ to 2 hours; a longer time does no harm. From the mercuric chloride transfer directly to 70 per cent. alcohol, to which a little tincture of iodine has been added. The tissue is washed in this till the alcohol ceases to lose color; then it is transferred to 75 per cent. alcohol, and finally to 95 per cent. alcohol for one day. Care must be taken to wash the tissue thoroughly in order to remove all the mercuric chlorde, otherwise needle-like crystals will form in the specimens. The tissue may be imbedded either in paraffin or collodion.

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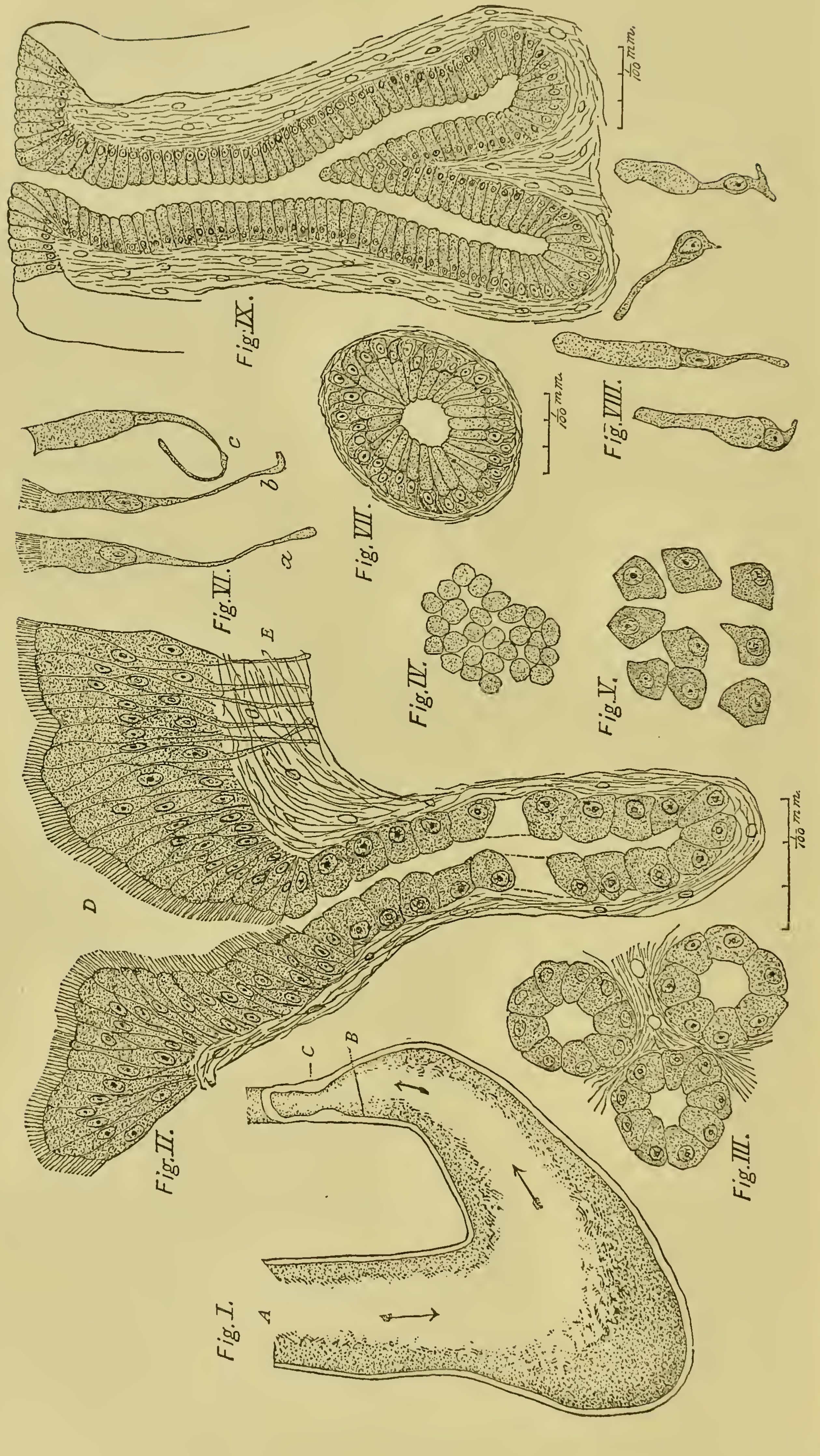
Fig. IX.
1/100 mm.
Fig. VIII.
Fig. VII.
1/100 mm.
Fig. VI.
a
b
c
Fig. IV.
Fig. V.
E
D
Fig. II.
1/100 mm.
Fig. III.
C
B
Fig. I.
A

Description of Plate.

The outlines of all the drawings, except Fig. I, were made by the aid of Abbé's camera lucida.

Fig. I.—This figure was made from a specimen slightly distended with alcohol. A portion of the walls have been cut away so as to show the extent of the ciliated epithelium and the direction of the ciliary currents.

A—Beginning of œsophagus.

B—Point where the cilia disappear.

C—Pylorus.

The direction of the ciliary currents is represented by the arrows.

Fig. II.—Cardiac gastric gland.

D—Mouth of the gland, showing the depth to which the ciliated epithelium extends into it.

E—Thread-like continuations of the epithelial cells extending into the mucosa.*

Fig. III.—Transection of cardiac gastric glands.

Fig. IV.—Ends of epethelial cells as seen in surface views of the mucous membrane.

Fig. V.—Isolated cells of a cardiac gastric gland.

Fig. VI.—Isolated cells from the surface epithelium of the stomach.

a, *b*—Ciliated epithelial cells.

c—Beaker cell.

Fig. VII.—Transection of pyloric gland.

Fig. VIII.—Isolated cells of the deeper part of a pyloric gland.

Fig. IX.—Pyloric gland, showing two glands opening into a single mouth.

(Figs. II, III, IV, V, VI, and VIII were all drawn on the same scale. Figs. VII and IX were drawn on a smaller scale.)

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*This was not actually demonstrated, but from the fact that the epithelial cells are much longer than the apparent thickness of the epithelium I believe that the figure gives a correct idea of the actual condition of things.

NOTE ON RESOLUTION OF AMPHIPLEURA PELLUCIDA BY CENTRAL LIGHT.

LYMAN DECK, M. D., Salamanca, New York.

The performance of the new apochromatic objective of Zeiss, claiming N. A. 1. 63, is causing so much comment among microscopists that I thought that an account of work done with an American objective might be of interest to your readers. I have an 1–12 homo. imm. objective made by Ernst Gundlach several years ago. It is marked Ernst Gunlach 1–12 Homogenous Immersion, 140°. T. U. E. F.

This objective is almost perfectly achromatic and has a remarkably flat field, and will work through cover glass 1–100 of an inch in thickness. It will work equally well with mirror in any position, from the axis to full 70°, with hemispherical lens in place, even resolving balsam *Amphipleura pellucida* with light so oblique that only one-fourth of the field is illuminated. Attaching this objective to a B. & L. universal stand with one-inch ocular and using light from a white cloud near the sun, admitted into a darkened room through a small aperture in the shutter and striking the center of the concave mirror, which was also carefully centered and reflected through the ⅛-inch aperture in the diaphragm, which was also carefully centered, using such a light I have repeatedly resolved the lines on difficult balsam-mounted *Amphipleura pellucida* as sharply and distinctly as an ordinary student 1–5 of 110° will show the beads on dry *Angulatum*. I have also seen on an 18-band Fasoldt test plate ruled from 5,000 to 120,000 lines to the inch; the 17th band, 110,000 lines to the inch, with light from an ordinary hand-lamp direct from the concave mirror. With sunlight I have seen the 120,000 band sharp and distinct. This is as fine a ruling as I have had an opportunity of examining. I cannot see the propriety of comparing the results attained by using slides of balsam *Am. pellucida* with slides of *Am. pellucida* mounted in a medium of high refractive index, as they are so much easier to resolve than balsam mounts. I have a slide of *Am. pellucida* mounted in a medium of high refractive index that is easier to resolve than *Frustulia saxonica* in balsam. With my 1–12 it is an amusingly easy feat to resolve the lines of *pellucida* on this slide with ordinary daylight direct from the concave mirror in central position. In fact, this slide is so easy to resolve that I have seen the lines on *pellucida* on it with my B. &. L. dry 1–5 of 130° professional series.

NOTE.—Dr. Deck exhibited at Detroit a slide of *Amphipleura pellucida* mounted by Miss M. A. Booth, Longmeadow, Mass., and said by her to be in balsam. The showing was made by the use of an ordinary small coal-oil lamp with flat wick placed about one foot to the left of mirror. The latter was used with plane side up, centrally placed, and a low-power objective inserted as a condenser. With this extemporized equipment the transverse lines on several specimens of the *Amphipleura* were seen by the Doctor himself and by the undersigned, though a half dozen others who looked failed to see them. Subsequently, during the same evening, Governor Cox also pronounced the diatom thus resolved.

Through the kindness of Dr. Deck, the writer took with him to his home the slide used in these trials. By means of photography the number of transverse lines on three shells were found to range from 94,000 to 95,000 to the inch. That from which the best negative was made had 94,200. Compared with the same diatom on a Möller's balsam-mounted test plate, the latter was found to be as easy to resolve as any observed on this slide of Dr. Deck's; but after earnest and repeated efforts no glimmer of resolution could be obtained by any possible device by central light from lamp. No. 19 on Möller's plate did come out, however, better than reported at the Detroit meeting with Zeiss' apochromatic 2 mm.

T. J. BURRILL.

A NEW DAPHNELLA.

C. M. VORCE, F. R. M. S.

In September, 1889, my attention was attracted to some fragments of an entomostracan observed in filterings from water of Lake Erie taken at Cleveland by Dr. L. B. Tuckerman. These fragments I at first thought to be portions of a Latona, but on examining some complete specimens of the creature from which they came, soon afterward furnished me by Dr. Tuckerman, they were seen to be specimens of a Daphnella, and prove to be specifically distinct.

I have named the new species *Daphnella Tuckermanni*, and append the following description: Carapace ovate, truncate posteriorly; head elongate, large, nearly as long as body; general appearance much like Sida or D. Wingii; foot as in the latter, with two long claws each with three smaller hooks, and two rows of minute teeth on the post-abdomen; caudal setæ extremely long, two-jointed, very slender, and slightly plumose near the very attenuate tip, set on a prominent bulbous cushion as in the genus generally; ventral margin of shell rounding, rimate, sparsely fringed, with setæ longest above and diminishing in length to the truncate margin; a row of 9 to 12 saw-like teeth or spines on the posterior ventral margin, ending at the angle formed by the truncation of the shell; these spines are intercalated with smaller slender setæ, and a row of fine, short, intra-marginal cilia extends thence to the posterior junction of the valves. Superior antennæ extremely long, setiform, obscurely two-jointed, and pectinate with fine short cilia on the under side, tip extremely attenuate; inferior antennæ very large and strong, rami unequal; first joint of inner branch with a terminal spine and one filament, outer joint with one marginal and three terminal filaments; first joint of outer branch with four marginal filaments, outer joint with six marginal and two terminal filaments and a terminal spine; all filaments of the antennæ very long, two-jointed, and profusely plumose; abdominal feet as usual, but with very long filaments, all finely plumose; intestine as in D. Wingii; eye large and placed as in the genus. The most striking peculiarities are the structure and excessive length of the superior antennæ and caudal setæ; length of body, .560; of head, .496; total, 1.05 mm.; length of superior antennæ, .528 mm.; of inferior do., .800 mm.; of inner ramus, first joint, .210; second joint, .064; terminal filaments, .432 mm.; of outer ramus, first joint, .160; second joint, .240; terminal filaments, .464 mm.; length of caudal setæ, .832 mm.; width of body, .448 mm.

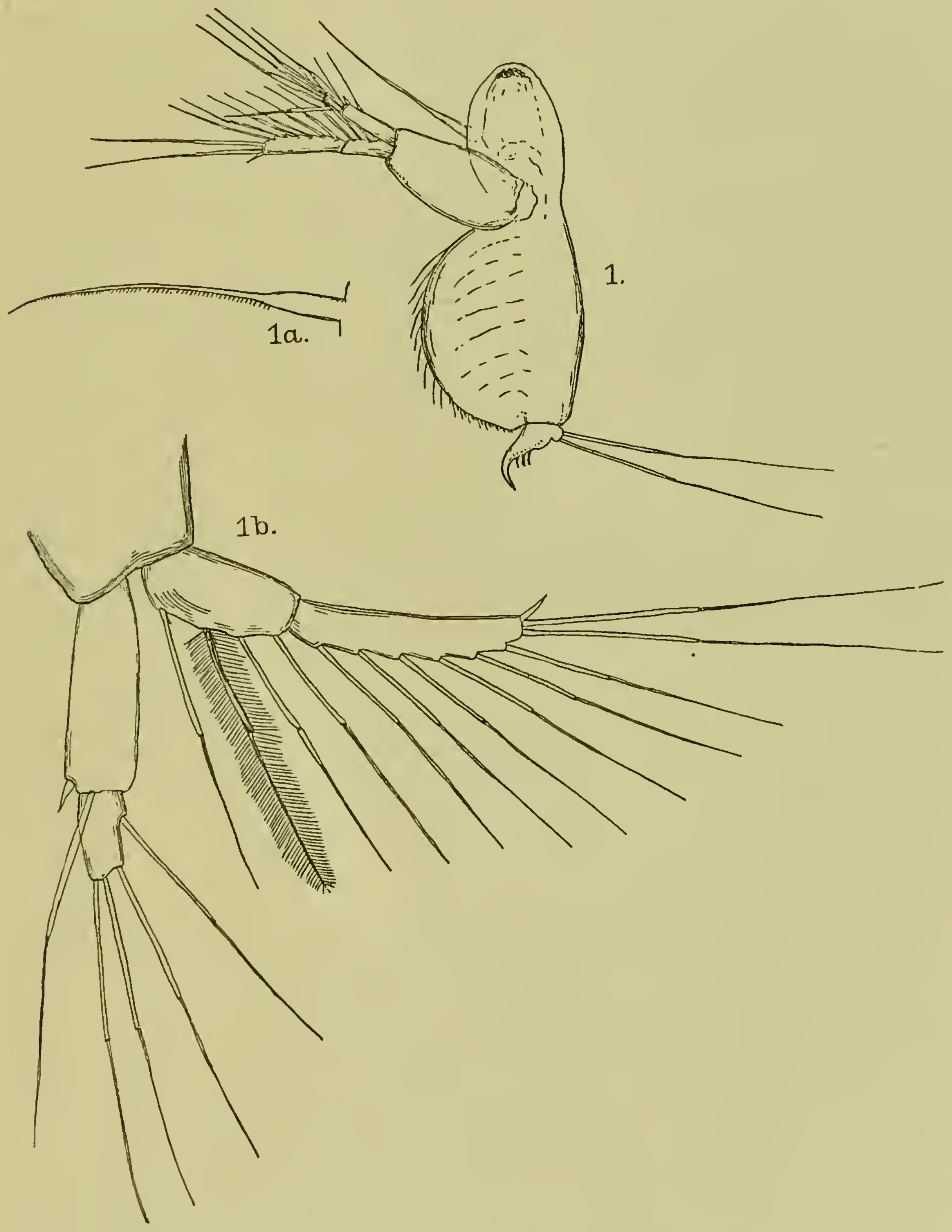


Fig. 1.--Side view × 40; *a*, superior antennæ × 63; *b*, rami and filaments of inferior, do more enlarged, one showing plumes × 100.

ADDITIONAL NOTES ON GOMPHOGASTER.

C. M. VORCE, F. R. M. S.

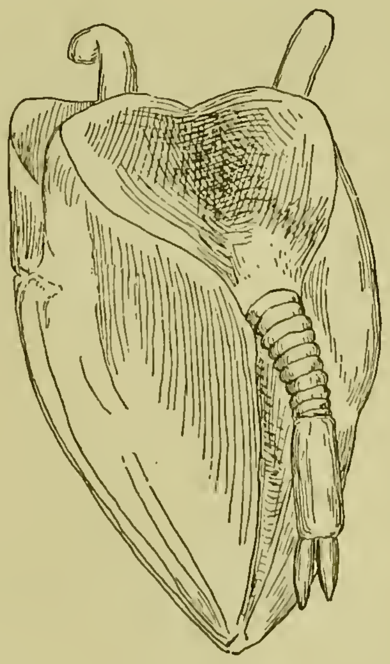
Observations made of this rotifer since the characters of the genus were published establishes the necessity for some slight corrections therein which it is the purpose of this paper to supply. This rotifer was first observed by the writer in the filterings from water of Lake Erie in July, 1882, and the single specimen then seen was figured and briefly described in the proceedings of this Society at the Elmira meeting, 1882, vol. iv, p. 187, pl. iii, fig. 76, etc., but not then named for the reason that I had not had sufficient time to thoroughly search the bibliography of the subject to determine whether the creature was really new. This point having been decided affirmatively before the Pittsburgh meeting, 1887, the new rotifer was there fully described and figured, vol. ix, p. 250, plate, page 253, and given the generic name of the title.

Although occasional examinations were made by me of lake-water filterings, no more specimens of this rotifer were found by me; but in the spring of 1889 a specimen was reported by Mr. Haskins, of Cleveland, and in the fall of 1889 Dr. L. B. Tuckerman, of Cleveland, who was then making continuous and systematic examinations of the lake-water filterings, observed a number of specimens, of which he at once informed me, and furnished me with both living and mounted specimens, from the study of which the following notes, additional to the description before published and in correction thereof, are derived.

This rotifer is figured in the supplement to Hudson & Gosse's Rotifera, pl. xxxiv, fig. 36, etc., and is described, substantially as in vol. ix of Proc., at pp. 57, 58 of the supplement, where it appeared, much to my regret, before I had seen the additional specimens obtained by Dr. Tuckerman, and before I was aware that it had ever been observed elsewhere. After the appearance of the Hudson & Gosse supplement, but before I had seen it, I learned from Professor Kellicott that a rotifer probably identical with Gomphogaster had been observed by Prof. C. L. Herrick at Granville, Ohio, and figured in vol. 1 of the Bulletin of Denison University, published in December, 1885. Although the figures there given differ in several respects from the specimens later observed by me, it does so no more, probably, than my own figures of the single specimen originally observed, which are now to be corrected; and from the figures and description by Prof. Herrick, taken together, I am satisfied that his specimens are generically the same as that observed by me and those obtained by Dr. Tuckerman, whatever may be the conclusion as to specific identity.

The lorica is not rigidly closed on the ventral apex below (behind) the foot, but the two lateral valves are joined by a flexible, tough skin, and can be closed together at will by the animal until their ventral edges are nearly or quite in contact. The sides are firm, and their ventral edges have a salient angle just opposite the insertion of the foot; hence, when the sides are drawn together, the foot must project in a forward direction, as seen in fig. 3, vol. ix, and fig. 36*c*, pl. xxxiv, Hudson & Gosse; also shown by the photograph, accompanying this paper, from one of the mounted specimens of Dr. Tuckerman. There is a transverse thickening of the skin before and behind the foot, forming a socket, in which the foot is seated. When distended, the lorica forms a shallow furrow along the ventral apex from the foot to the posterior end of the shell, in which furrow the foot generally trails along when the animal is swimming, the toes reaching nearly or quite to the end of the shell.

The accompanying cut is made from a pen drawing from photographs.



The sides of the lorica curve outward gracefully to the upper dorsal corners; below (behind) the angle at the foot the sides sink in and are somewhat hollowed. On the dorsal surface the lorica has a deep, transverse, chevron-shaped fold, doubly plicate, behind which the back is occupied by a number of longitudinal folds, all converging to the acute posterior apex of the shell; of these folds two on each side of the median line are the deeper and more distinct, throwing the back into distinct ridges between them. Forward of the chevron-shaped dorsal fold the back is flattened, smooth, with two diverging sub-median ridges extending to the frontal margin of the shell, and between these and the lateral margin the dorsal surface is considerably depressed or hollowed. From the ends of the transverse dorsal fold a cleft extends on each side to the dorso-lateral corner of the dorsal plate, which is joined to the sides at this cleft merely by the soft and yielding integument of the body. The frontal edge of the dorsal plate is slightly incurved, making the corners prominent, and extends beyond the sides. As a result of this structure the portion of the dorsal plate forward of the transverse fold is movable hinge-like thereon, and the anterior corners flexible sidewise, so that it may be folded forward and much reduce the exposure of the cephalic extremity of the body, the sides being at the same time drawn in at their anterior extremities, so as to nearly close together the lorica, tortoise-like. All the folds of the lorica are more or less dilatable, but I have never seen any of them completely effaced. The folds on the back and that along the dorso-lateral angle are the most persistent.

In the original type specimen the areolation of the lorica was distinct and marked. In the later specimens this characteristic is quite variable, being strong in some and very faint in others.

The foot is wrinkled for about two-thirds its length from the body; then succeeds a stout, smooth joint, from which proceed two stout toes laterally appressed; so that in life, when viewed from the side, they appear as a single toe. When the lorica is shut up, as it may be expressed, the foot points forward and the toes reach nearly or quite to the anterior extremity of the body, and thus guard the otherwise unprotected cephalic extremity.

The mastax is large, pyriform, transversely barred, with a protruding angle within the cleft and on the ventral side. The mastax is placed high up and the trophi are moderately strong.

The eye is single, dorsally placed, and within the margin of the shell as the animal is normally extended. Whether the body can be so extended as to bring the eye outside the lorica I cannot say from observation, but it is probable that it can.

The two protruding organs (antennæ or tactile processes) are curved, clavate, and slightly bifid at their extremities. Besides being partially retractile they can be curved down and thus apparently shortened.

The trochal disc is still not satisfactorily made out for the reason that it is only fully expanded while the animal is swimming freely, at which time it is impossible to keep the disc in focus of a sufficiently high power long enough to make out the details of structure. At the least indication of confinement, as by getting among conferva or the flocculi of vegetable débris always present in lake filterings, the animal draws in, partially closing together the lorica, and either lies quietly with its retracted cilia lazily working or kicks lustily with its foot, and thus spins itself about without protruding its disc, until, of a sudden, it starts off with a dash, as if touched up with a sharp point, and usually has to be patiently searched for before it is again found.

The generic characters published should, therefore, be modified as follows: Foot wrinkled at base, jointed toward extremity; toes two, large, laterally appressed; eye single, dorsal; mastax pyriform, moderately large.

As to the specific identity of the various specimens thus far observed, it may be asserted confidently that all the specimens observed at Cleveland are of the same species, *G. areolatus*, notwithstanding the variation in the degree of distinctness of the areolar markings. As to the specimens observed by Prof. Herrick, some doubt may be based upon the fact that the anterior margin of the dorsal plate was notched, as stated by him and shown in his figures, while in *areolatus* it is smooth and gracefully curved. In all other respects, including the areolation of the lorica, Prof. Herrick's specimens agree with *areolatus*, and it may be noted that in size his specimens agree exactly with the original type specimen of 1882, while all those later observed at Cleveland were noticeably smaller.

DEFORMED DIATOMS.

JACOB D. COX, A. M., LL. D.

The general uniformity in the recognized species of Diatoms and their adherence to beautiful geometric forms are so marked that we are apt to overlook the fact that irregularities and deformities are not rare. Notes of a few of these have been published, but I believe it may be instructive for students and observers to keep a record of the abnormal variations from type. Such notes will enable us to make some broader comparisons of the character and extent of these "sports," and possibly to draw some sound conclusions as to the causes of variation and its bearing upon the distinction of species.

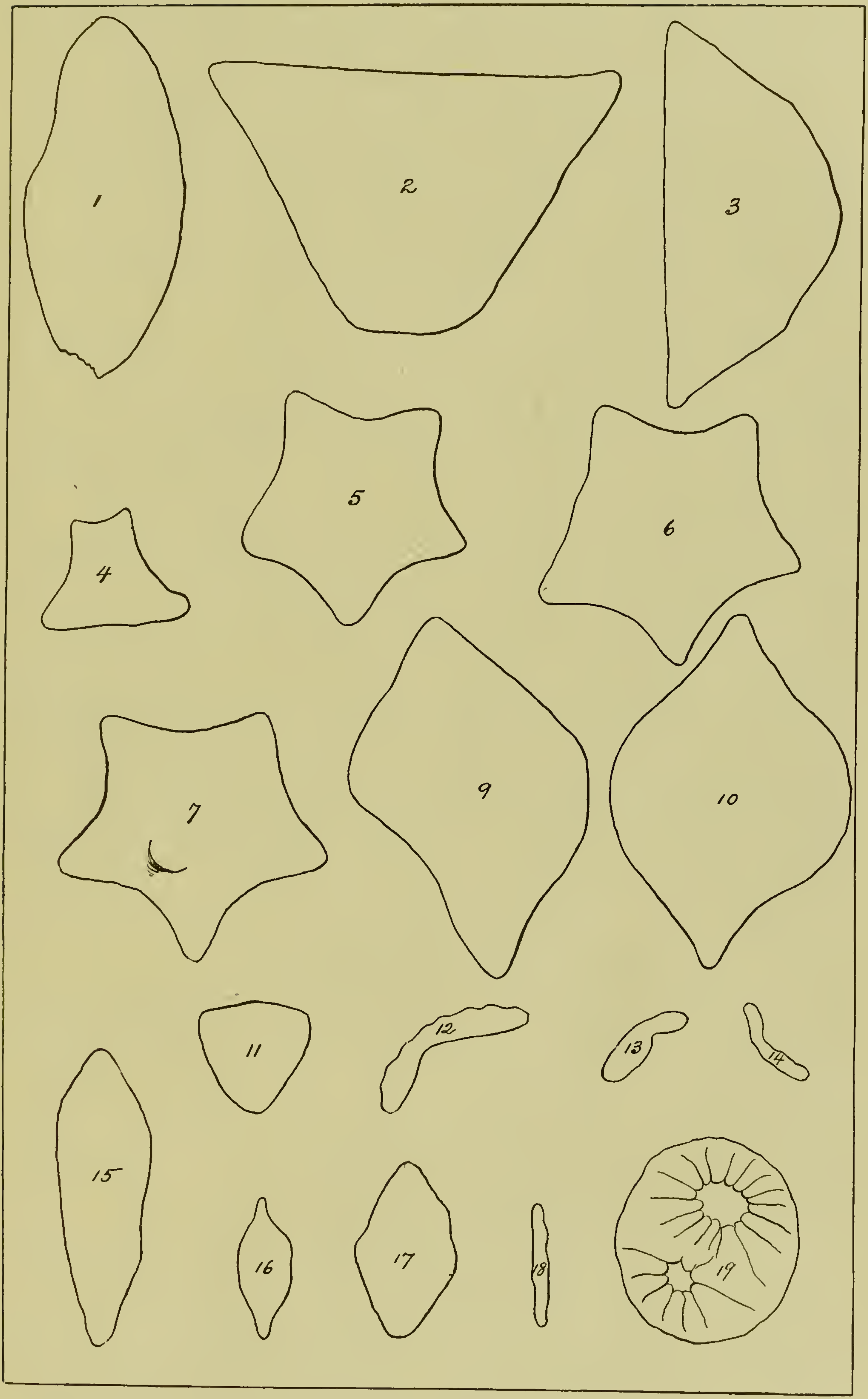
The following memoranda refer to examples which I have met from time to time in my own investigations, and which I have photographed. Prints of these are deposited with the curator for the use of the Society. I have provisionally arranged them in three classes: 1. Those having indented or deformed outlines; 2. Those having a double or multiple center in the scheme of marking; 3. Those having the marking unsymmetrically varied. The prints have amplification × 700.

I. INDENTED OR DEFORMED OUTLINES.

1. *Navicula lyra, Ehr.* Santa Monica, Cal.; fossil. One side boldly indented in a large, easy curve.

2. *Triceratium favus, Ehr.* Maranham Island, Pacific Ocean; recent. One angle is wanting, two sides being joined by a curve, making one diameter one-fourth less than the others. The process is present at the deformed angles, but is unsymmetrically placed.

3. *Same species,* from same gathering. A similar deformity carried further. The form almost semi-lunar, but the curve is not regular. The third process is wanting.



This plate is traced from photographs and reduced one-half, and the numbers of the figures correspond with the numbers of the paragraphs, all but No. 19 being in the first class.

4. *Triceratium robustum, Grev.*= *T. sculptum Shadbolt?* Calvert Co., Maryland; fossil. One of the angles is replaced by two smaller ones, giving an irregular trapezoidal form to the shell.

5. *Triceratium formosum, Brightwell, var. pentagonalis, Grun.* Samoa Islands, Pacific Ocean; recent. One of the indentations between the points of the star is nearly obliterated, and the two points connected by an irregular curve.

6. *Same species and variety*, from same gathering, similar deformity, but the line connecting two points of the star is more nearly straight, showing a tendency to revert to the triangular form of the type.

7. *Same species and variety*, from same gathering. The deformity here consists of a large umbonate protuberance in the center of the shell, its surface covered by the regular marking.

8. *Same species, typical.* San Luis Obispo, Cal.; fossil. A typical triangular form, but with the central protuberance noted in No. 7.

9. *Biddulphia rhombus, W. Smith.* Cuxhaven, North Sea; recent. One side deformed by a large, irregular indentation.

10. *Same species*, same gathering. Similar deformity but less pronounced.

11. *Same species.* Triangular form = *Triceratium striolatum, Ehr.* A group of three specimens, of which two are the triangular forms, with regular and moderately convex sides; the other specimen has one such side, but the other two curving into each other in a similar way to No. 2 above. The third process is wanting, but in its place are two spines, such as appear on the sides of the regular *Biddulphia* form. Variation in this species is so common that it would be easy to make a series of photographs showing the gradual passage of the *Biddulphia* into the triangular form.

12. *Eunotia diadema, Ehr.* Charleston, S. C.; recent. The concave side irregularly curved, making the width of the shell much less in one place than elsewhere. The convex side is regularly eight-toothed.

13. *Eunotia monodon, Ehr.* Loka deposit, Sweden; fossil. A deep irregular indentation on concave side near one end, making the shell roughly comma-shaped.

14. *Eunotia arcus, Ehr.* Dolgelly earth, Ireland; fossil. A similar (but less marked) irregularity. In a recent gathering at Cincinnati, Ohio, these *Eunotias* are found with so irregular reversed curves as to resemble wriggling earth-worms in outline. A specific distinction has been improperly based on this peculiarity.

15. *Licmophora ovata, Ehr.* Santa Monica, Cal.; fossil. A large, shallow indentation on one side, giving a gracefully varied outline.

16. *Rhaphoncis amphiceros, Ehr.* Savannah, Ga.; recent. One side of the shell a regular curve, the other very irregular.

17. *Cymatopleura elliptica, Bréb.* France; recent. The outline of the shell on one side irregularly wavy.

18. *Grammatophora marina, Kg.* Algoa Bay, Africa; recent. One side wavy, unsymmetrical.

II. DOUBLE OR MULTIPLE CENTER IN THE SCHEME OF MARKING.

19. *Mastogonia actinoptychus, Ehr.* Richmond, Va.; fossil. Elliptical form, with two distinct central spaces from which the costae radiate.

20. *Stictodiscus californicus, Grev.* San Luis Obispo, Cal.; fossil. A fine circular specimen with two centers, the radiating costae approximately regular on the outer limbs, but anastomosing in the space between the two centers.

21. *Navicula samoënsis, Grun.* (=*Nav. palpebralis, Greg. ?*) Samoa Islands, Pacific Ocean; recent. In one half of the shell, longitudinally, a second median nodule appears, with striæ radiating from it in all directions.

22. *Biddulphia balæna, Ehr.* = *Triceratium formosum, Brightwell.* Spitzbergen, Arctic Sea; recent. A large *Biddulphia* form, in which the delicate marking radiates from four or more distinct points as centers.

III. MARKING UNSYMMETRICALLY VARIED.

23. *Coscinodiscus robustus, Grev.* Pabellon de Pica, S. A.; guano. The areolation is so irregular as to have lost all hexagonal form. Its strong margin, robust habit, and its occurrence among typical specimens of similar size, &c., fix the specific relation. A similar example in same species was found in the Santa Monica deposit.

24. *Arachnoidiscus ehrenbergii, Bailey.* Santa Monica, Cal.; fossil. The radiating costæ irregularly wavy and anastomosing near the center, where they also become fainter and hardly traceable.

25. *Actinoptychus heliopelta, Grun.* Nottingham, Maryland; fossil. A specimen with five elevated rays. These are plainly distinguished at the central zone, but the undulations flatten out soon,

and the outer part of the shell is quite flat and evenly areolated. The rim with its irregularly scattered spines is typical.

26. *Triceratium affine, Grun.* Samoa Islands, Pacific Ocean; recent. The areolation is so irregular as to lose entirely its hexagonal character; this is the case all over the shell. Its size and outline and its occurrence among numerous typical specimens show its relations. A second example has been noted on another slide from same locality.

27. *Amphitetras antediluviana, Ehr.* Kiel, Baltic Sea; recent. The central part of the shell is irregularly areolate with a tendency to two centers. The typical forms common in same gathering have the central areolation evenly concentric.

28. *Navicula elliptica, Kg.* Crane Pond, Mass.; recent. The striation is irregularly wavy, especially at the ends.

29. *Epithemia turgida, Kg.* Berlin, Germany; fossil. The costæ and areolation are so irregular as to destroy all pattern in one-half the shell and nearly so in the other.

30. *Nitzschia scalaris, Ehr.* Sodertelge, Sweden; recent. In one part of the shell the keel is distorted, and instead of it appear two nodules on the face of the shell with irregularly radiating striæ.

Whenever I speak of striation I mean to be understood that it is resolvable into dots similar in character to those of the typical forms of same species.

In the examples which I have placed in classes II and III above, we see the distortion occurring by the miscarriage or interruption of the vital force which tends to produce the symmetrical sculpture of the shell of the diatom. In class I the cause of the irregularity would seem to be external and mechanical. It results, apparently, from the conditions of growth within a strong silicious box. When fission occurs the envelope of the new cells is at first flexible and is often found very slightly silicified, but it acquires rigidity as the deposition of silex goes on.* Under these conditions, if we suppose that the diatom is better supplied with nutriment than common and its circumstances are such as to make more vigorous growth, the valves of the new cell may tend to grow larger than the parent in all directions, and being confined by the strongly silicified coat of the mother cell, its own more flexible wall may fold upon itself and make a wavy irregularity of outline, such as is shown in some of the examples above noted.

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* See Study of *Isthmia nervosa* by the author, in Am. Journal of Microscopy, vol. iii, p. 97 (1878).

The examples of irregular wavy outline suggest also the conclusion that if, in the circumstances last supposed, the diatom were one whose shell presented alternate strong and weak points in its circumference, the indentations resulting would also be regular and we should have a crenulate form of a shell normally smooth in outline. My own observations have led me to think it probable that crenulated forms are thus originated. Once produced, further fissiparous division would tend to perpetuate the peculiarity, since the growing force in the new cells would force the flexible walls outward to fill the matrix.

It would not be till the process of conjugation is reached that the resumption of the normal type could reasonably be expected; consequently it would be premature to conclude that the crenulation is a specific distinction or even the mark of a lasting variety until it is proven that after conjugation the new series of diatoms retains that form.

Considerations of this sort may assist us in reducing the enormous catalogue of species in the *Diatomaceæ*, a "consummation devoutly to be wished."

THE COSCINODISCEÆ.—NOTES ON SOME UNRELIABLE CRITERIA OF GENERA AND SPECIES.

Jacob D. Cox, A. M., LL. D.

Description of Plates 4 and 5.

Fig. 1.—*Coscinodiscus subtilis*, Ehr., craticular form. Fossil. × 1050. Petersburg, Va.

" 2.—*Cos. oculus-iridis*, Ehr., outer surface of rosette. Fossil. × 1300. Nottingham, Md.

" 3.—*Cos. oculus-iridis*, Ehr., inner surface of rosette. Fossil. × 1300. Nottingham, Md.

" 4.—*Cos. excentricus*, Ehr. Fossil. × 750. Nankoori Island.

" 5.—*Cos. radiatus*, Ehr., triangular form. Fossil. × 400. Beach Haven, N. J.

" 6.—*Actinocyclus Ralfsii*, var. *Monicæ*, Grun. Fossil. × 800. Sta. Monica, Cal.

" 7.—*Cos. radiolatus*, Ehr. (*C. curvatulus*, Grun.). Fossil. × 1000. Moron, Spain.

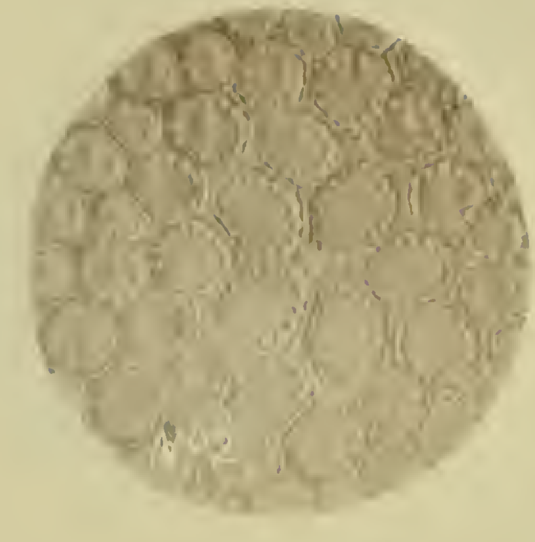
" 8.—*Cos. radiolatus*, Ehr., craticular plate. Fossil. × 600. Barbadoes.

" 9.—*Actinocyclus interpunctatus*, Brightwell, var. Fossil. × 1000. San Luis Obispo, Cal.

" 10.—*Actinocyclus interpunctatus*, Brightwell, var. Fossil. × 800. Sta. Monica, Cal.

If I needed a text for what I have to say it would be hard to find a more appropriate one than the following extract from the last annual address of Dr. C. T. Hudson, President of the Royal Microscopical Society of London.

"The multiplication of species," he says, "is a crying evil, and the exasperating alterations of their names in consequence of changing classifications is another. The former, of course, is mainly due to the difficulty (no doubt a very great one) of determining what shall be a species and what a variety." In illustration of this he refers to the fact, mentioned by Darwin, that "one expert has made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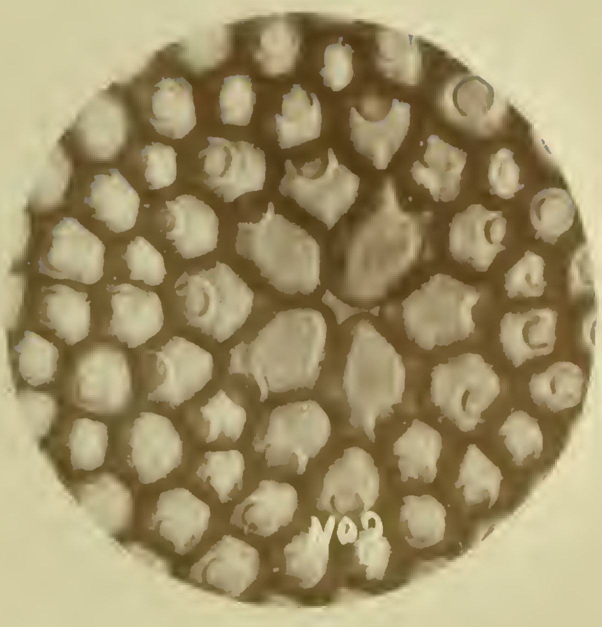
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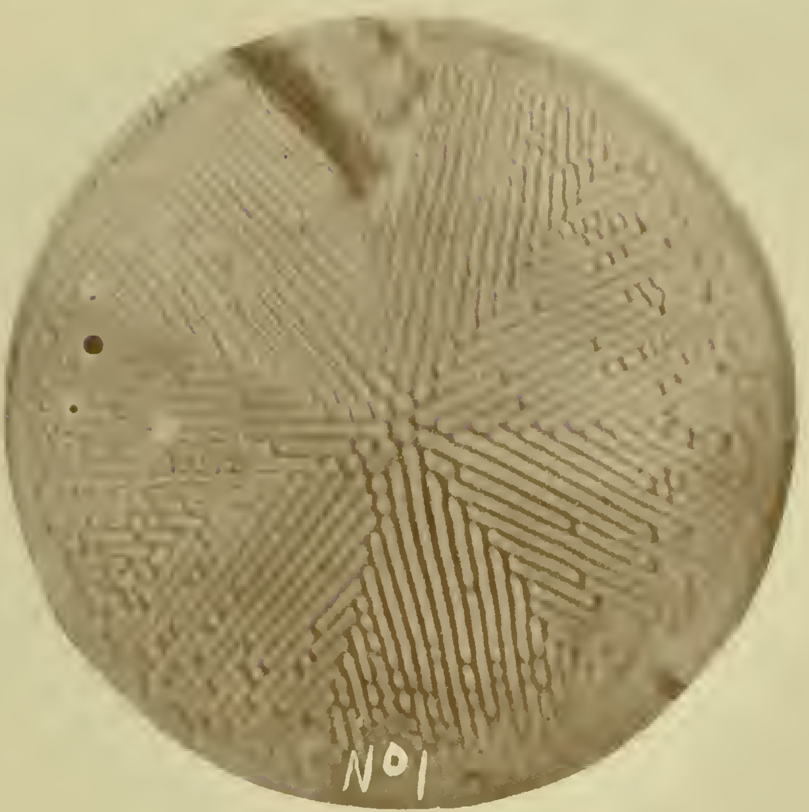
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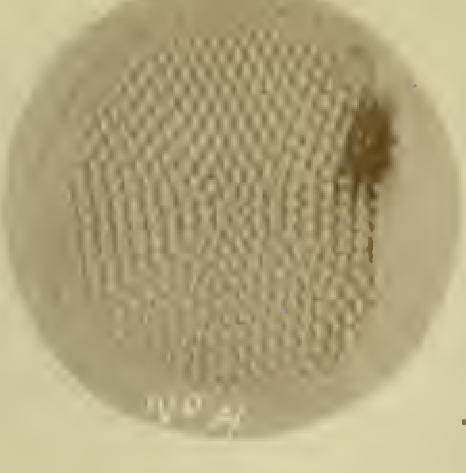
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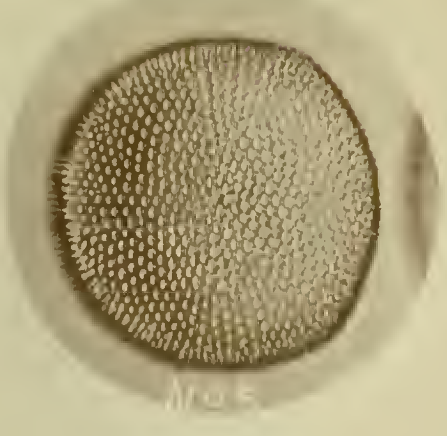
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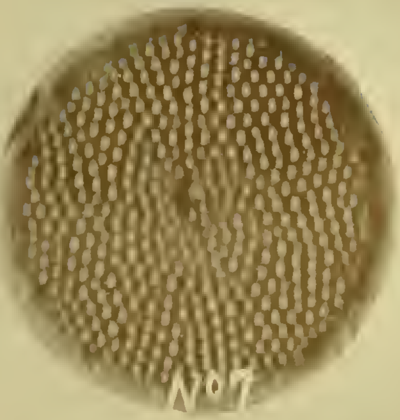

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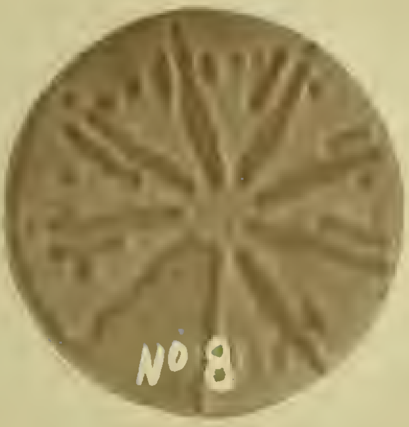

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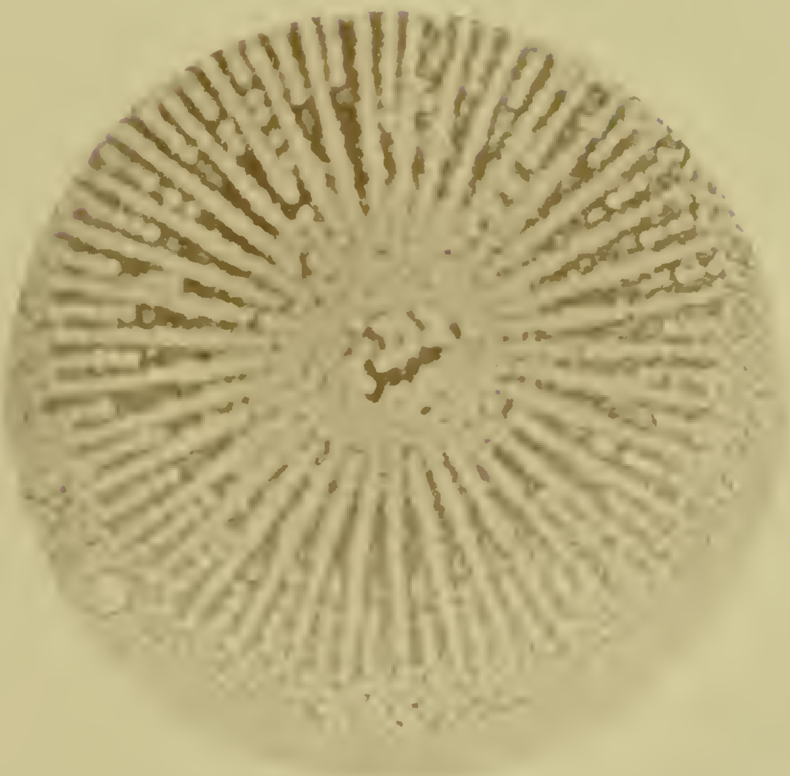

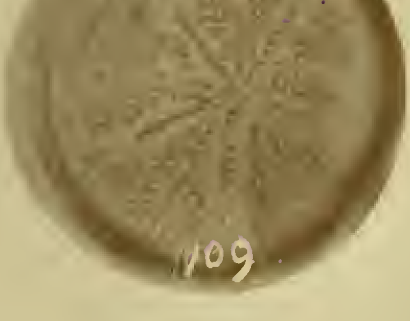

No 9

No 10

no fewer than thirty-seven species of one set of forms which another arranges in three." *

As long ago as 1863 Dr. Greville, in his monograph on the genus Auliscus, recognized the danger of his own large additions to the *Diatomaceæ*, but spoke of his work as an accumulation of materials which could " only be accomplished at the risk of encumbering both genera and species, more or less, with a provisional nomenclature." That he really regarded his new lists as more or less provisional was shown by his quoting from Professor Walker-Arnott (another distinguished worker in the same field) the statement that " the time may ere long arrive when what are now called genera or sub-genera will alone be considered species, and another Linnæus be requisite to reduce the chaos to order." †

If the multiplication of species of diatoms seemed portentous in 1863 what shall we say of it now, when the Habirshaw-Chase catalogue contains about ten thousand, and the additions have never been so rapid as since that catalogue was prepared? It is true today, as it was thirty years ago, that the students of microscopical botany who have been most familiar with the life history of the diatoms have generally been the most conservative in the admission of new species. Such investigators as Van Heurck on the continent of Europe, Kitton in England, Gregory in Scotland, and H. L. Smith in this country are examples of strong opposition to the rush for new names. Professor Smith's Conspectus was a bold reduction of the overcrowded list of genera, and has been generally accepted as a work of needed reform, though at first it seemed to many too radical in its retrenchments.

I do not propose to discuss the question, what is a species? I wish to deal with a more practical matter by calling attention to some things which seem to me to have been unsoundly assumed to be criteria of specific distinction. If I am right, the foundation will be laid for a considerable diminution of the list of species of *Coscinodiscus* at least, and, as I think, will help in the application of a similar criticism to other genera. I shall only assume it to be a maxim of the law of classification that a new distinction of species shall not be based upon trivial, uncertain, or fleeting characters in the specimen under examination, and that those species, so called, which cannot stand this test shall be cancelled.

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* Journal R. M. S., February, 1890, p. 134.

† Transactions R. M. S., 1863, pp. 39-41.

What I wish to say will arrange itself most readily about seven typical forms, as follows:

1. *Actinocyclus ehrenbergii, Ralfs.*
2. *Coscinodiscus subtilis, Ehr.*
3. *Cos. radiolatus, Ehr.*
4. *Cos. lineatus, Ehr.*
5. *Cos. radiatus, Ehr.*
6. *Cos. centralis, Ehr.*
7. *Cos. Marginatus, Ehr.*

Leaving out of view some irregular and doubtful forms, these seven may be taken as types of the natural divisions into which the genus *Coscinodiscus* separates, the distinguishing marks of these species being the most obvious, persistent, and trustworthy criteria for a subdivision of the genus. I shall defer, for a little, the statement of my reasons for including *Actinocyclus ehrenbergii*. They will appear as the discussion proceeds.

In all these we find those marks of the genus which may be considered as the generic pattern or norm, viz., a disc, circular, of shallow arch (nearly flat); the disc surface covered with alveoli of uniform character (round or hexagonal), the hoop (connecting zone) smooth and narrow as compared with the diameter of the disc. It is not necessary for my present purpose to consider the negative marks of distinction from other genera. All of these marks may be subject to exceptions and variations, for we are dealing with living things, not geometrical figures, and a living thing is never strictly geometric in figure, however it may approximate to this. In scientific classification, the exception may fairly prove the rule when the rule is based on sound induction. It does not destroy the faith of a naturalist in his classification, for instance, if he finds triangular forms classed both with those regarded as normally circular and those normally oval or lozenge-shaped, provided the different triangular forms have characteristics which indicate their closer relation to the circular norm on the one hand, or to the oval on the other.

Passing, then, from the generic marks of the seven species I have named, we find that they differ from each other by definite and easily distinguishable characters.

1. *Actinocyclus ehrenbergii, Ralfs.*

The disc is divided into compartments or segments by radial lines of alveoli, at the marginal end of each of which is a small spine.

Each compartment is filled by a fascicle of lines of alveoli of which the middle line is radial and the other lines parallel to the middle one. The outer rim is beveled or curved sharply beyond the spines, and upon this are more numerous lines of finer alveoli, making decussating as well as radial striæ. A " pseudo-nodule " is near the margin.

2. *Coscinodiscus subtilis, Ehr.*

Marking of the disc like the last, except that the radial lines between the fascicles are omitted and the parallel lines of the wedge-shaped fascicles are lengthened till those of adjacent fascicles touch. The small spine is at the marginal end of the middle line of the fascicle. Beveled rim beyond the spines similar to last. No pseudo-nodule.

3. *Coscinodiscus radiolatus, Ehr.*

Marking of the disc similar to the last, except that the fascicles are each formed by a radial line with parallel lines on one side of it only, the fascicles being all similar to each other and symmetrical. The small spine is at the end of the radial line forming the side of the fascicle.

4. *Coscinodiscus lineatus, Ehr.*

The disc covered with alveoli in straight lines parallel to a diameter and arranged in *quincunx*, so that when the round alveolus becomes hexagonal the surface of the disc resembles a true honeycomb, the most prominent lines in appearance being parallel to the given diameter; a loose circle of small spines marginal or intra-marginal.

5. *Coscinodiscus radiatus, Ehr.*

Marking of the disc made up of hexagonal alveoli in radial lines, which, starting from a central rosette, bifurcate as they go outward, so that there are rarely continuous straight radial lines of alveoli. In robust specimens the upper surface of each alveolus has a dotted appearance, caused by finer secondary pits or marks.

6. *Coscinodiscus centralis, Ehr.*

Marking of the disc of hexagonal, round or sub-quadrate alveoli, which, starting near the center, go outward in continuous radial lines, new radial lines being intercalated as room is made by the outward divergence. Secondary marking in robust specimens: In large examples the central zone is thin, and the outer zone relatively robust and thick.

7. *Coscinodiscus marginatus, Ehr.*

Marking of the disc sub-hexagonal alveoli, without traceable scheme of radiation, but in largest specimens approaching *Cos. radiatus.* Margin strongly recurved and produced, so as to make a more and more deeply cup-shaped valve in each new frustule formed by fission, the small valves being thus most deeply cupped in a given brood and the large valves most flat.

The last of this list has the most disputable claim to specific distinction, but the balance of reasons seems to incline in favor of recognizing it.

My inclusion of *Actinocyclus* in the genus *Coscinodiscus* is based upon the fact that it is so plainly one of the series of fasciculate forms that we must either make a genus of each of them or include all under one. In considering what I regard as insufficient generic criteria I will begin with this:

A. The so-called pseudo-nodule of *Actinocyclus* is less important as a generic mark than the other characteristics which are identical with the fasciculate *Coscinodisci.*

In classification we certainly ought to proceed from more widely distributed marks to those less so. In all the more noticeable characters, *Actinocyclus* agrees with *Coscinodiscus subtilis.* The difference is analogous to that which was once supposed to be a good specific distinction, viz., the number of fascicles. The fasciculate character is common to all. The number of fascicles differs. If this difference were persistent, it might be a good basis for establishing a variety, but it is so plainly a subdividing of the fasciculate class that it cannot be regarded as other than a distinction of inferior grade. For this reason the pseudo-nodule with the radial lines of alveoli separating the fascicles may be taken together as making a good distinction of species but not of genus.

The earlier idea was that the pseudo-nodule had some important distinctive physiological character. No evidence of this has been found, and it is now commonly regarded as a mere shell marking, not a process. It is less important as a distinction than the processes of *Cos. concinnus,* and hardly more so than the central smooth space of *Cos. perforatus,* once supposed to be an opening in the shell.

The occasional appearance of the pseudo-nodule in other *Coscinodisci,* as in *Actinocyclus curvatulus, Janisch,* is evidence of relationship between species (as far as it goes); but I agree with Schmidt in regarding the last-named form as having the more numerous and strong marks of *Cos. curvatulus, Grunow*, the presence of the pseudonodule being the exceptional fact in the diagnosis. (See Schmidt's Atlas, pl. lvii, fig. 31.)

B. Color is an untrustworthy mark of species.

Color in diatoms of all genera is among the most variable and illusive of marks. It depends upon the lamination, which produces iridescence in these forms as in mother-of-pearl; but lamination proceeds by additional deposition of the silex through the whole growth of the diatom. In its earliest life, when the silicification is imperfect and the fibrous vegetable character of the tissue preponderates, there is no iridescence, even in *Actinocyclus*. Vigorous, strongly silicified specimens shine with brilliant prismatic colors; but we find decay, due to chemical action, often setting in after death, and extensive fossil deposits will give us innumerable *Actinocycli* as pale and void of color as cotton cloth. Again, the color depends on the magnifying power used in examining the shell. One which is brilliant under a "two-thirds" objective will be colorless under an "eighth." Still again, the tint and character of the rings will be varied by a slight tilt or inclination of the plane of the shell as well as by the robustness of the lamination and the slightest variation in regular curve of the surface. Old and partially corroded diatoms are often found with the laminæ partly removed. *Actinocyclus* is very liable to this, the thickened patches being often almost black, giving rise to false species and varieties.

C. The number of fascicles is no mark of species.

This seemed so obvious and clear a natural distinction that it was inevitable that early observers should base new species upon it. Ehrenberg ransacked astronomy and mythology for new names to the five or six score species of *Actinocyclus* thus made, but naturalists gave a sigh of relief when Ralfs swept them away. We do not know more than that this is a most variable character. We sometimes find very few fascicles and sometimes very many in shells having such relation to each other as to tempt us to make the difference more significant than it is. Investigation has, however, detected a difference in the number of fascicles in the two valves of a single frustule, and the accepted generalization of Ralfs has thus been justified. Yet there has been a curious persistence in treating the distinction as worth something in an extreme case. If we take the scheme of the formation of *Actinocyclus* fascicles and consider the effect of increasing the number and correspondingly diminishing the lines in each fascicle it is evident that we come at last (or in the next to the last step) to a fascicle of three lines, the middle one a radius and the other two very short and near the margin. At first blush we seem to have only an arrangement of alternately long and short lines; but if the radial lines *between* the fascicles are taken into the account we shall see that the form which I have photographed in plate 5, fig. 6, is the regular form of *Actinocyclus* with the fascicles thus reduced to a minimum size. To it a number of different names have been given, and it appears in Van Heurck's atlas as *Actinocyclus ralfsii, var. Monicæ Grunow* (Van H., pl. cxxiv, fig. 3). *Coscinodiscus biradiatus, Greville,* and *Coscinodiscus scintillans, Greville,* are either the same or varieties of the same. In the last named, as exhibited in H. L. Smith's "Species Typicæ," No. 99, I have found the pseudo-nodule unmistakably present in instances enough to decide the question, though the surface of these shells is so irregular, owing to the scaling off of laminæ, that the inconspicuous pseudo-nodule may easily be overlooked. My note book for 1884 shows that I reached this conclusion after an examination then made.

My observation leads me to suggest the following as the lines of *tendency* in variation among the fasciculated forms: *Actinocyclus* varies more in the number of fascicles and the reduction of the number of lines in the fascicle; *Cos. subtilis* varies more in the regularity of the fascicle. This, of course, is offered as only true in a general way.

D. The so-called subulate spaces in *Actinocyclus* are not marks of distinction of species.

These spaces are those which occur at the inner end of each line of a fascicle, bounded on one side by the next longer line of the fascicle and on the other by the radial line between the fascicles. It is clearly illustrated by plate 5, fig. 10, which is from a very symmetric specimen in the Santa Monica, Cal., deposit. If the marking of the specimen is loose, the spaces will be relatively large; if compact, they will be relatively small; if stoutly silicated, they will sometimes look almost or quite black and be very prominent. The marking is often so compact that the radial lines between fascicles are wavy, as if they were pressed into these spaces, almost obliterating them. Variations in these respects may be found in the examples of any pure gathering of any one of the supposed numerous species of *Actinocyclus*. An observer happens to find a specimen more than usually noticeable in the one or other extreme and names it as a new species. It seems to me utterly unreliable as a specific distinction. The curious fate of Brightwell's *Actinocyclus interpunctatus* is worth noticing. He described sufficiently well, according to the distinctions of genera then received, an *Actinocyclus* with unusually prominent double lines of subulate spaces. His reference to the confusion between *Actinocyclus*, *Actinoptychus*, and *Eupodiscus*, then common, and especially what he says of Ehrenberg's use of the number of fascicles, as a specific distinction, leaves no room for dispute that he had a true *Actinocyclus* under his eye. He says* "The true species of this and the preceding genus we believe to be few, while Ehrenberg, falling into the extraordinary error of viewing every variation in the number of rays or septa as a species, has made a fresh nomenclature and a new description of nearly all the species necessary." It was this suggestion, no doubt, which Ralfs, in the following year, carried out in the new edition of Pritchard's Infusoria (1861), by condensing the hundred or more species into *Actinocyclus ehrenbergii*. Even Brightwell's figure was fairly good as figures then went, before photographic methods, and his references to habitat (in Monterey earth, amongst other places) help identify his specimen; yet Grunow identified it as his *Hyalodiscus stelliger* of the Moller type plate. H. L. Smith showed this last to be no *Hyalodiscus* at all, but a *Coscinodiscus* (*C. interpunctatus, H. L. S.*†). Prof. Smith's conclusion as to *H. stelliger* has been generally received; but instead of following him in correcting the error by reinstating Brightwell's *A. interpunctatus*, this has been dropped by latest systematists, and the form has been named half a dozen times over as a new discovery. In the series of photographs which I deposit with the custodian of the Society as supplementary to the plates published, No. 1, of *Actinocyclus ehrenbergii* from a recent gathering from the Oregon coast, Pacific Ocean, very fairly shows what Brightwell intended to describe as "double rays running from the center to near the circumference, the rays composed of short broken lines." With this compare Nos. 2, 3, 4 of same series.‡

The changes in diatom marking not only, but in form, which occur in the process of fissiparous generation are little known as yet, but there is a growing opinion that they are often great. I believe that in all the *Coscinodisceæ*, at least, variations in this respect occur as the new broods of diatoms grow smaller, until the departure from the type has been considerable; but when conjugation again takes place, two (perhaps) deformed irregularly marked specimens will produce a symmetric, fair, and typical form again. In plate 5, fig. 9, is a photograph of a small example which might well be exhibited as specifically different from the types we have been considering, but it is my opinion that its variation has been the result of the process just spoken of.

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*Jour. of Mic. Science, vol. 8, p. 94. (1860.)

† Am. Jour. of Mic., vol. 2, p. 100.

‡ As it has been impracticable for the Society to present photogravure plates of more than a few of my photographs, the engravings will be referred to by number of plate and figure; the others simply as photos No. 1, &c.

E. Considerable changes of form may occur without becoming the ground of new species.

This, which is closely connected with the last subject, may be passed with brief treatment, as the statement is generally accepted. Some extension of its application has been hinted at in my paper on Deformed Diatoms (*ante*, p. 182), and I will only add a suggestion of the manner in which such forms as *Palmeria hardmaniana, Grev.* or *Euodia gibba, Breb.* (= *Hemidiscus cuneiformis, Wallich* *) may temporarily arise. If, from any mechanical or other obstruction in its growth, one of the fasciculate forms of the *Coscinodisci* should have its two valves become adherent on one side during its early and unsilicified state, the hardening of the tissue by the silex would force its growth into the *Euodia* form, so closely resembling a bivalve; but in the process of further fissiparous division the character of this mode of reproduction forbids and prevents the return to the type by reason of the inflexible character of the box in which the self-division takes place. Consequently we can have no return to the type till conjugation takes place, and an indefinite number of generations of this deformed character may succeed each other meanwhile. Under such circumstances, it would seem to me inadmissible to call the new form anything but a temporary variation till the frustule formed after conjugation shall be shown to keep its peculiarities. Even then, if side by side with it the typical form is found, it can hardly be said to be more than a permanent variety. Yet, on what seems to be such a distinction, we see, in the example given, two new genera founded.

F. Sparseness of alveoli is often misleading as to pattern of marking and is not reliable as a specific distinction.

The misleading effect of sparseness is analogous to that already mentioned in the case of fascicles reduced to a minimum number of lines. The scheme is less visible, though easily traced when the clue is given. In photo. No. 6 is an example of *Actinocyclus* from a recent gathering at the Samoa Islands, Pacific Ocean, in which the scheme of marking is that of three-lined fascicles, but the sparseness of the dots makes it difficult to trace the pattern. In some of the smaller specimens of other fasciculate forms the difficulty is even greater, and I believe that these are often regarded as lacking a definite pattern when larger specimens (less removed from the conjugate frustule) would show it clearly.

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* Supplementary Series of Photo's, No. 5, from Moron (Spain) deposit.

An analogous difficulty is found when the marking is hexagonal and made up of large alveoli. In such cases the scheme or pattern, as shown by the inner plate or lamina of the valve, is that of sparse dots, but the hexagons above are not strictly symmetrical or even. There is, consequently, a great difference in the apparent marking when we focus upon the outer surface (showing the hexagonal marking) or upon the inner surface, showing the pattern of the dots. Photographs Nos. 7 and 8 are taken in this way from a *Cos. radiolatus*, Ehr. (= *C. curvatulus*, Grun.), from the Santa Monica deposit.

G. A striated margin is often apparently present or absent, as more or less of the beveled marginal zone of the fasciculate forms is shown.

I have often found examples of *Actinocyclus* and of *Cos. subtilis* in which the beveled edge before mentioned was a little flatter than common. In these the appearance of a separate scheme of marking for the margin is illusory. It can hardly be contended that a little more or less bevel at the margin is a specific difference. If the hoop or connecting zone of the frustule is uncommonly stout, the valve will often show a circular line within the extreme circumference and parallel to it, where the attachment of the hoop has left its trace. In such cases the real difference is in the stoutness of the hoop, which is as variable a quantity as the other. Photo. No. 9 is from an *Actinocyclus* from the Santa Monica deposit, showing this apparently strong and striated margin. No. 10 is another from same kind, a little less regular in pattern, apparently in consequence of a wave or roll in the surface of the shell showing more plainly. Nos. 11 and 12 are prints taken at different focus from another specimen of the same group, showing the subhexagonal marking on the surface and the general scheme at a deeper focus.

H. New species have often been based upon different valves of the same frustule.

Every student of diatoms knows that in some species the valves are dissimilar; *e. g.*, in *Cocconeis*, *Achnanthes*, &c. I have found frustules of *Cos. subtilis* whose valves varied in the number of fascicles, and I have found others in which one valve was *C. symmetricus*, and the other the irregularly fascicled form which some systematists consider the true *C. subtilis*. These specimens occurred in recent gatherings from the coast of Florida (Jacksonville), and from Hudson river (Poughkeepsie). That they occasionally pass from one form to the other is conclusive proof that the difference is neither specific, nor that of permanent variety. Photo's Nos. 13 and 14 are of specimens from the Poughkeepsie water supply and very well illustrate the variation, though these are not valves of the same frustule. From general size, appearance, and association in the same gatherings, such relations may be fairly surmised, and experience often justifies the suspicion. The rejection of the number of fascicles, &c., as a specific distinction was first based on this general suspicion. Prof. H. L. Smith has expressed his suspicion that *Cos. concinnus* (*Eupodiscus Jonesianus*, *Grev.*) and *Cos. nobilis*, *Grun.* are differing valves of one frustule,* and the opinion of so experienced and sagacious an observer may safely be accepted, provisionally, at least.

I am quite satisfied in my own mind that the so-called *Coscinodiscus excentricus*, *Ehr.*, is only a form of *Cos. lineatus*, and often a valve of a frustule of which the other half is the latter type. The cumulative character of the reasoning is something on this wise: The essential character of the marking is the same. They are found constantly associated and often so identical in all the subordinate characters of size, robustness, color, &c., as to have the same relations to each other in these respects that specimens of any common pure gathering have. The difference is one easily accounted for by what we know of diatom structure and growth *without* assuming difference of species. The peculiar honeycomb marking of *C. lineatus* is dependent on flatness of surface, and its regularity will be disturbed if the surface is convex. In this case a wedge-shaped segment or gore must be taken out to allow the surface to curve, and the bringing of the edges of the gore together will disturb the parallelism of the marking exactly as we find it in *C. excentricus*. It thus appears that the difference is not one of scheme of marking, but of the application of the pattern to a flat or convex surface. This in itself ought to settle the question of *specific* difference, for nobody has pretended to make slight variations of curvature of surface a basis of species. But this is not all. The very closely allied *Stephanopyxis corona, Ehr.*, is known to have its two valves of different curvature, the circlets of spines being further from the margin in the more convex valves, as they must be to fulfill their office of holding the frustules together in the earlier phase of their life history.* In the fair, large specimens, which figure in Möller's Typeplate as *Systephania corona* and *S. diadema*, we have in the flat valves the regular *C. lineatus* with the marginal circlet of spines strong and sometimes confluent. I have again and again found these in frustules of one valve of each so-called species in the Richmond deposit. Photo. No. 15 shows a typical *Cos. lineatus* from the Nankoori deposit. Nos. 16 and 17 are from *Systephania*, in the Richmond deposit. I cannot think them either generically or specifically distinct. The spines will be mentioned in next paragraph. I regard *C. excentricus* as identical with *C. lineatus*, and *Systephania* as, at most, a variety of the same, the tendency of the valves of a frustule to vary in convexity being characteristic of the whole group.

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* Am. Jour. of Mic., vol. ii, p. 103

In plate 4, fig. 4, published herewith, is an exceptional specimen of *C. excentricus*, which looks at first sight very like *C. subtilis*, the surface being divided into seven fascicles or compartments. Closer attention, however, will show that the arrangement of the hexagons is that of the *C. lineatus* group and not the other. The gore lines in the so-called *excentricus* may be one or several, but they rarely come so near to a *symmetrical* irregularity as in this example.

I. Marginal or intra-marginal circlets of spines are a very variable character.

I have spoken in the early part of this paper of the normal form of the species under consideration, and do not doubt that in perfect types the small spines are marginally placed, as I have indicated. Their office is the strengthening of the hold of the frustules upon each other in the movements and strains to which these are subject before becoming "free;" but the spines vary greatly in prominence, in number, and in regularity of spacing. They have very often been overlooked because very small. I have very rarely failed to find them on the margin of the fasciculate species. Even when they cannot be seen in focus, if the microscope tube is raised a little they will be detected by the *coma* or shadow exaggerated in this way. Plate 5, fig. 7, is an example of the way in which this appears, in a case where the spines were nearly or quite invisible when in sharp focus. The genus *Odontodiscus* which Ehrenberg based on these spines was rightly cancelled by H. L. Smith. They are not always present in the fasciculate species, though rarely absent. They are often found in *Cos. radiatus* of the most robust varieties, as *C. oculus iridis*, &c. They are not always symmetrically placed in the fasciculate species, though they usually are. Sometimes there will be a spine at the margin of only the alternate fascicles. I have notes of some instances in which they were numerous around one quarter, say, of the circumference of a *C. subtilis*, and absent elsewhere. Such are exceptional cases.

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* Van Heurck's Atlas, pl. lxxxiii, *ter.*, fig. 11.

The more numerous the spines are, the more irregular they are apt to be. This is very marked in *C. lineatus*, in which the circlet is apt to be interrupted. In robust growth the circlet is often fimbriate or confluent. These variations are so evident in undoubted examples of a given species (or even variety) that it is impossible to base specific distinctions upon them. The photographs above referred to illustrate this. An additional reason is thus found for relegating *Systephania* to *Cos. lineatus*. The tendency to a central (or interior) circle of spines in *C. lineatus* is shown in the fact that one or two small spines are very often found near the center of the valve without others, and this in specimens from very widely separated *habitat*.

What I have thus said of spines is of much more wide application than to the *Coscinodisci*. On *Triceratium favus*, *T. spinosum*, &c., they are very variable. On *Eupodiscus radiatus* from the Florida coast I have often found complete *alæ* surrounding the valve, and in their nature a confluent growth of spines.

K. The rosette in the center of *Cos. radiatus*, &c., is not a mark of species.

This follows from the structure of the rosette, when we ask what causes it. The alveoli are the quasi-pores through which endosmose takes place to support the life and growth of the plant. They resemble a bubble between the siliceous laminæ of the valve, the surrounding solid parts being of the full thickness of the bubble. When the alveoli crowd upon each other the solid walls (in this genus) become hexagonal in the areolation. When either the shell is very thin or the alveoli are comparatively sparse, a central space of clear silex is left, such as Ehrenberg made the mark of the species *Cos. perforatus*. Photo. No. 18 is taken from a specimen in the Holland's Cliff (Maryland) deposit, in which all the alveoli are round dots, and the radiation illustrates the description I have given of *Cos. centralis* as a type. From this small form, photographed, the series runs through *Cos. centralis* to *Cos. gigas*. In the intermediate stages are numerous examples, in all our Maryland and Virginia deposits, of valves areolate partly in dots and partly in hexagons, one limb of the disc being usually (in such cases) different from the other. Such irregular forms have been classed as new species under name of *Cos. obscurus, Schmidt,* &c. As the robustness of the shell increases and the alveoli become hexagonal, the laminæ are of course separated by these cellule-walls. Each inner alveolus spreads more or less into the central blank space, making a shallower extension of the hollow in that direction. In very robust and thick shells of *Cos. radiatus* (*e. g.*, those known as *C. oculus iridis, C. omphalanthus,* and *C. asteromphalus*) the separation of the laminæ and the extension of the inner circlet of alveoli have gone so far that little or no blank space is left, the rosette leaves meeting in the center of the shell.

It is well known that the under lamina of the shell is not areolated into hexagons like the upper, but the "eye-spot" (a circular thin spot in the silicated membrane) is seen at the bottom of each alveolus as we focus down to it. In the rosette or "anthus" these eye-spots are found in the outer part of the elongated alveoli, approaching no nearer to the center in *C. radiatus* than in *C. perforatus*. The appearance of the rosette is, therefore, the index of the thickness and robustness of the shell, and in itself has no other significance. Plate 4, figs. 2 and 3, are photographs of the rosette of *C. oculus iridis, Ehr.*, from the Nottingham, Maryland, deposit; in the first the focus was upon the dotted upper surface, and in the second upon the eye-spots beneath in the same shell. My note books of 1885 are full of notes and measurements of these rosettes, in which overwhelming evidence was collected of the passage of the small round alveoli into hexagons and these into the "anthus," the "eye-spots" approaching no nearer the center of the shell.

The series of forms which I class under *Cos. centralis* have, however (besides their peculiarity of radiation), a tendency to thinness of the central part of the shell in the largest valves, which are presumably nearest in generation to the conjugate frustule. *Cos. radiatus,* on the other hand, grows stoutest in the largest specimens. It is easy to make a series beginning with *Cos. perforatus* in round dots, passing by almost infinitesimal degrees through forms partially or wholly in hexagons to *Cos. centralis,* with shallow "anthus" or rosette, and with alveoli increasing in size outward, and from this to *Cos. gigas* of the Richmond earth, in which the rosette has disappeared, the central alveoli diminishing to mere points in the delicately thin shell, but increasing gradually outward to the few rows of very large and deep alveoli which are found at the margin. This tendency to thinness of center in this species is best seen in a field where they are numerous and viewed with a low power. Photographs Nos. 19 and 20 of my supplementary series show this clearly. They are from slides of the Richmond earth, which is very rich in *Cos. gigas* and its congeners.

The rosette will be found whenever the shell is robust in comparison with the size and number of the alveoli. It is found in *Cos. Concinnus* and other forms of fine areolation, as well as in the great specimens I have named. I am thus justified in concluding that the rosette has no *specific* significance, since we find it appearing, disappearing, and changing form in the same species with the robustness of the specimen and the amount of silication.

Before passing to the next topic I invite attention to the analogy between the numerous examples of *Coscinodiscus* with one limb or segment in dots, whilst the rest is in hexagons, and the supposed case by which I have accounted for the *Euodia* and *Hemidiscus* forms. In the former we have examples of valves manifestly thinner in one portion than elsewhere. Increase this difference and the conditions will be present for the adhesion which I have supposed to be the occasion of the lunate forms.

L. *Craspedodiscus* is not generically or specifically distinct from *Coscinodiscus*.

The typical forms are *Cras. elegans*, Ehr., and *Cras. coscinodiscus*, Ehr. The former is found associated with the large *Cos. radiatus* (*C. oc. iridis*) of the Virginia and Maryland earths. The latter is more widely distributed. The remarkable size and general structure of *Cras. elegans* and its general agreement in all qualities of structure and form, except the roll near the rim, would naturally suggest its close relationship to the gigantic *Coscinodiscus*, in whose company it is found. The suggestion gains weight when we find that large specimens of *Coscinodiscus* from all parts of the globe show a tendency to a similar wave or roll. Sometimes nearly the whole disc will be thin and flat, but two or three rows of alveoli around the rim will be more robust and show a pronounced upward curve. Numerous examples of this sort are found in Möller's slides from Saldanha Bay guano. It is in examining the Richmond earth itself, however, that the evidence grows conclusive. In a gathering made by Mr. Peticolas, in Buchanan street, in Richmond (1881), the *Cos. oc. iridis* is found with the roll or wave in every degree of progress. In these it is often on only one-half the shell or less, the other half having the normal marking of *Coscinodiscus*, and even in the roll the form of the alveoli is little, if any, distorted. An example of this is shown in a broken shell, Photo. No. 21. The roll is rarely perfect all around the shell. There will nearly always be found a place where the wave slides gradually into the disc, as may be seen in Schmidt's figure (Atlas, pl. lxvi, fig. 1). When the roll is boldly formed the alveoli are lengthened radially to suit the increased distance, being thus distorted from the normal hexagons into lozenge shape.

In the smaller forms of *Craspedodiscus* it is usually the thin depressed center in which the alveoli are distorted or aborted. Here the process is evidently similar to that which I have several times mentioned. The upper and under films of the valve are not fully differentiated, and the alveoli are partly degraded or suppressed. I look upon it as one of the forms which result from the crumpling or crowding of the shell in its early stage of growth, when the daughter cells are too vigorous for the silicified matrix in which they are formed. The weak part of the valve will give way in such cases. Schmidt's plate lxvi is a valuable study, showing how this force acts in different species of *Coscinodiscus*. I believe that the same cause produces *Porodiscus* and the roll in *Actinocyclus* which must also be denied the importance of a specific distinction. *Cos. excavatus*, Grev. (Sch. Atlas, pl. lxv, fig. 1), is, in my opinion, only another development from a similar cause, and the whimsicality of our diatom nomenclature is strikingly shown in the fact that the latter has been retained in the genus *Coscinodiscus* whilst the others have been ranked as independent genera. If one roll or depression makes a genus, *Cos. excavatus* should have that honor, "*ter quaterque*"! Photo's Nos. 22 and 23 illustrate different stages in the smaller *Craspedodiscus*.

M. The occurrence of two thin places in the rim of *Coscinodiscus* is not a mark of species.

This character is figured in Schmidt's Atlas, pl. lxiii, fig. 13, and on it *Cos. biangulatus*, A. S., is founded. In my studies of *Cos. radiatus* I have found that whenever this species reached a certain robustness the two thin places (a little more than 90° apart) may always be detected in the rim. Since 1885 my note books overrun with evidence of it. I traced it in the so-called *Cos. oculus iridis* of the Jutland cementstein, in *Cos. radiatus* of the North Sea (Cuxhaven) gatherings, in all the Virginia and Maryland deposits. Photo. 24 shows this appearance when the rim is focused upon, and No. 25 when the surface of the valve is in focus. These specimens are what I regard as the large form of *C. marginatus*, *Ehr.*, where it is hardly distinguishable from *C. radiatus*, except by the continued thickness of the rim. Plate 4, fig. 5, is an example from Beach Haven, N. J., the northern extension of the Maryland fossil deposits. In it the progress toward a triangular form is shown in further development. This is very interesting as indicating an explanation of the manner in which this disc form becomes triangular. In the Petersburg, Va., earth I have found a perfect triangle, but of much finer marking, in which two of the corners were marked by the common blank spot, but the third lacked it, the marking being there continuous over the angle. It is significant also that in *Cos. concinnus* the prominent processes are placed as these undeveloped processes are in *C. radiatus*. I look upon this character, therefore, as probably present in all varieties of *C. radiatus*, becoming noticeable when the forms are robust. It becomes, consequently, a natural mark by which to connect and diminish species, not to enlarge the list.

N. Confluence of alveoli into larger ones is not a mark of generic difference.

This has been generally accepted in the *Navicula* tribe when the original distinction in genus of *Pinnularia* from *Navicula* has been cancelled. The reason for it was not that which may now be given, for the true nature of the alveolus was not at first recognized. Now we know that what we call the costate form of *Pinnularia* simply means that the rows of alveoli have become confluent into one, the elongated "eye-spot" being found in the inner film smaller than the outer film of the alveolus, but keeping the relation of parts and of office which exists in the circular or hexagonal alveolus. The same is seen, in a less degree, in the long alveoli of many of the didymous forms of *Navicula*.

I suspect that something similar has occurred in the so-called craticular plates of *Naviculæ* (*e. g.*, *Surirella craticula*, &c.), though these have not yet been proven to have double films in the thin parts which make the sculpture of the figure. Although a little beyond my present subject, I will refer to some examples of craticular plates found by our late lamented member, Dr. Sloan, and his co-worker, Dr. Clapp, of New Albany, Ind. These accurate observers found in considerable numbers compound frustules of *Navicula cuspidata*, in which there was this regular series: The two exterior valves were typical *N. cuspidata*, with produced ends and fine transverse striæ. Within these was a frustule, of which both valves had the strong radiant marking of *N. radiosa*. Innermost of all was the single craticular plate (*Sur. craticula*). They found these so frequent in a gathering of diatoms near their home that they mounted some slides in their unexcelled method, and I had the pleasure of verifying their description. Passing by the bearing of this on the problem of variation of species, I call attention to the fact that, so far as I know, the craticular plate is always single, never a frustule. If we suppose that fission had begun, but the new frustule aborted, and the two valves unite in one silicified plate in which the alveoli never develop, but thin places roughly simulate the normal pattern, we may get at least an intelligible idea of the way in which such a plate may be formed consistently with the common laws of diatom growth and structure.

Plate 5, fig. 8, shows a plate found in the Barbadoes deposit which agrees pretty well with the characters of *Actinogonium* (see Van Heurck's Atlas, pl. cxxvii, fig. 8). In this instance the marking is that of a small but coarse *Cos. radiolatus* (= *C. curvatulus*), in which the dots had become confluent in shallow, thin canaliculi or grooves. The radial lines have on one side of them shorter lines, diminishing in length, following the pattern of the *Coscinodiscus* in examples, which have fascicles of few and stout lines of dots. Van Heurck doubts the claim of his figure to be a new genus, thinking it a craticular plate of *Asterolampra*. It seems to me more closely connected with *Coscinodiscus*.

But in plate 4, fig. 1, I give the reproduction of a photograph from a shell found in the Petersburg, Va., deposit, which seems to me to throw fresh light on the subject. The figure is that of *Coscinodiscus subtilis* with six symmetrical fascicles of numerous lines, in which the alveoli seem to have become confluent, with a few exceptions, which remain, as it were, for proof of the normal pattern from which it is a variation, and of which it is a diagram prepared by nature. It seems to me very similar in character to the last object described, though more elaborate. Its peculiarities would, no doubt, seem to many worthy of a new generic name; but I do not at all believe it is anything else than an exceptional form of *Cos. subtilis*. In Truard & Witt's diatoms from Jeremie, in Hayti (T. & W., pl. 5), are examples of a similar confluence of alveoli in triangular forms, which are very interesting for comparison with this.

I had intended to add a few lines upon the valueless character (for purposes of specific distinction) of apiculi near the center of the fasciculate forms, as in *Cos. stellaris*, *Roper*; *Symbolophora trinitatis*, *Grun.* (not *Ehr.*) of the Möller Type Plate, &c., and also of the irregular variability in number and spacing of the central alveoli in *Actinocyclus* and the other fasciculate *Coscinodisci*: but I have filled as much space as seems proper for a single paper.

My general purpose has been to show that with all the wonderful simulation of geometric forms in diatoms they still, like other vegetables, have a large variability and a free application of the normal patterns to changing conditions of growth; next, to point out the numerous directions in which our increasing knowledge of the anatomy and physiology of these plants shows that supposed specific differences are accounted for as merely individual peculiarities of growth or condition, or, at most, as temporary varieties; lastly, to emphasize the fact that the enormous multiplication of species destroys the scientific value of classification and bars the way to a rational system. Instead of waiting for fuller knowledge, observers have run headlong races to publish new genera and species upon a single specimen or even fragment of a specimen. Much of the later so-called systematizing has been only an "*enumeratio simplex*" of individuals, from which, as Stuart Mill in our youth taught us, there is no logical induction.

It seems to me a duty to say that it is practically impossible to identify species with any certainty among the diatoms with the published means now at command. Artificial keys have become complicated and delusive. The searcher with a specimen before him soon runs upon vague and ambiguous distinctions, and is fortunate if he does not find a dozen described species to either of which it may with equal plausibility be attributed. Life is too short and human memory too finite for any one mind to master the catalogues of species which multiply by fission as rapidly as do the diatoms themselves under favoring conditions. I venture to say that no living student in this department can be sure that he has a new species (much less a new variety) before him. The impossibility of being certain leads an observer to solve his doubts in favor of his own discovery, and the same object is named and renamed.

To illustrate this, let me refer to my reasons for making *Cos.*

radiolatus, Ehr., rather than *Cos. curvatulus, Grun.*, the third, and *Cos. marginatus, Ehr.*, the seventh of the typical forms treated in this paper. The early observers did not fully resolve the markings of their specimens, and therefore often missed a full description. This does not absolve us from the duty of determining what they had under their microscopes. Not to do so is to leave scientific lumber behind us to block the way of all future investigators. To solve the doubt in favor of any probable and definite form to which figure and description will reasonably apply is better than to ignore them and add a new name of species to the list. The deposits and gatherings examined by the early microscopists can generally be duplicated, and no work is more profitable than earnest effort to follow their steps with our better instruments, sympathetically and not too critically. Treated in this way, Ehrenberg's work, as reported in *Berl. Bericht*, and Prof. Bailey's in Silliman's Journal, throw mutual light on each other. The history of *Cos. subtilis* and *Cos. radiolatus* show that these species were regarded as closely allied, though the fascicles of neither were accurately described. Bailey identifies them as found in the Bermuda (Nottingham), the Richmond, and the Petersburg deposits. Following this clue and carefully noting the original criteria of other *Coscinodisci* in the same deposits, I have satisfied myself that the two types, *subtilis* and *radiolatus*, are those which were distinguished in fascicles as I have described them. Some years ago I named as *Cos. subtilis* var. *molaris* (mill-stone) the *radiolatus* form with straight lines in the fascicles, and deposited photographs with this Society and the R. M. S. of London, accompanying a paper on Broken Diatoms. This name should be cancelled as well as *C. curvatulus*, Grun., unless I am wrong in attributing it to Ehrenberg. Ehrenberg noted several forms under names of *Cos. marginatus*, *Cos. concavus*, *Endictya oceanica*, &c. (found also by him in our American deposits), which, for reasons already hinted at, I believe to be forms of one species in different stages of the life-history of a diatom-lineage. If I am right, the type is the form which results immediately from conjugation, and is the larger, flatter, most regular in marking, and with least recurved rim. This is *C. marginatus*, Ehr., and I do not think it specifically distinct from *C. robustus*, Grev., and some others under different names.

The seven typical forms to which I have confined my attention in this paper represent several hundred species as given in the catalogues. The criteria on which the long list is built (ignoring numerous mere multiplications of names) are nearly all those which I have discussed. I earnestly believe that the whole should be reduced to these with comparatively few varieties of each. I must not be understood as including all *Coscinodisci* and *Actinocycli* in this assertion; but the several hundred which have the typical marks I have attributed to these seven. This gives my measure of the reduction which, I think, would result from a thorough and scientific revision of the whole family.

Whoever will turn to Dr. William Gregory's paper on "Shape or Outline as a Specific Character in the Diatomaceæ," in the Transactions of the Royal Microscopical Society for 1857 (Q. M. J., vol. iii, p. 10, *Tr.*), will find an argument with a telling object-lesson on the useless multiplication of species, even at that day. It is in the effort to imitate the spirit of that eminent naturalist that I have prepared these remarks. Lest I should be misunderstood, I will add, in closing, that the abundant material presented, with admirable figures from reproduced photographs, in such works as Schmidt's Atlas, Truard & Witt's Jeremie Diatoms, and Pantocsek's Hungarian Diatoms are invaluable and trustworthy material for our study. If we shall only establish true criteria of species based on wider familiarity with the life-history of each, we may hope ere long to remove the reproach that our department of investigation is being literally gorged with undigested material.

The Necrology.

FRISBY T. NEWCOMER, M. D., M. A. S. M., F. R. M. S.

Dr. E. F. HODGES, Indianapolis, Ind.

Born in Hagerstown, Md., December 10, 1828, and died in Indianapolis, Ind., September 1, 1889. He was educated at St. James College and at Marshall College, at Mercersburg, Penn., at which institution he graduated in the spring of 1848. His medical education was obtained at the University of Pennsylvania, where he took the degree of Doctor of Medicine in the spring of 1851, having also taken a summer course in Blockly Hospital. Fully equipped as a physician, he selected the city of Indianapolis, then a town more remote from his home than Denver would be to-day, and in the fall of 1851 definitely settled there in the practice of medicine. He did what few professional men do to-day, decided at first where he should live, and without experimenting with success, simply made himself one with the community for all time, living and dying after nearly 40 years of practice in the place of his original selection. His practice grew from the first, and he was always prominent among his medical brethern as a successful and conscientious physician, while among his friends and patients (and it may be mentioned that President Harrison was of these) no one was more cordially esteemed and revered. He was never the physician for a certain class of persons, the members of a given social caste, of a church or brotherhood, but was always the friend and counselor alike in the home of luxury and the hovel of the poor. Wealth he neither sought nor obtained, but his life was a pathway strewn with unnumbered generous and kindly acts which see the light to-day and will live in the hearts of very many persons. He was married early, a year after settling in Indianapolis, to Miss Sarah Ellen Irwin, who survives him with the children born to them, Mrs. B. D. Walcott, Miss Nancy Newcomer, and Mr. George Newcomer. During the war he was commissioned by Governor Morton as examining surgeon and served for three and a half years, frequently being ordered to the field hospitals after a battle. He was subsequently appointed examining surgeon for the Pension Department and served until President Cleveland came into office. He was post surgeon for the United States Arsenal at Indianapolis for more than 20 years—indeed, until the end of his life. He was a man with great enthusiasm, one who retained to his death in a remarkable degree the characteristics of youth. As light hearted as a boy, even when professional responsibilities pressed him closely, he never seemed to lose either elasticity or courage, and his warm-hearted presence was always thought by his friends and patients to contribute quite as much as his medical acumen of skill to their cure. In all that pertained to science, his inquiring form of mind found especial delight. He was for years an authority on floriculture, but it is with the class of diatoms that his name must be especially identified. In the selection, cleaning, and mounting of these he developed astonishing skill and made himself famous wherever these objects are known and studied. Some of his arranged preparations of the diatoms are unsurpassed in excellence anywhere. A member of the American Society of Microscopists from the organization to the time of his death, he was one of its most faithful supporters. He was elected First Vice-President in 1886, and contributed a paper to the annual meeting of that year upon his specialty. He was always prominent in the working session, contributing his time and skill generously to the brethren assembled. During the last year of his life he was elected a Fellow of the Royal Microscopical Society of Great Britain, an honor well deserved.

Our Society will feel his loss severely, for among its members are numbered some of his warmest friends. Few of the brethern ever saw him without interest and none knew him without esteem.

EUGENE PINCKNEY.

AMOS W. PATTON, D. D., Aurora, Ill.

Mr. Eugene Pinckney, a member of the American Society of Microscopists, died at his home in Dixon, Ill., in the month of February of the present year. He was a graduate of Princeton College. He was by profession a lawyer. He was a man of extensive information and broad culture. His library was one unusually well selected. He was a careful and earnest student, most thoroughly investigating every subject he took up. He became a member of the American Society of Microscopists in the year 1881. As a lover of scientific investigation, he applied himself to the study of biology with great enthusiasm. He invested in an elegant outfit, sparing neither pains nor expense to secure the very best instruments he could secure. His collection of microscopic preparations was one of the finest in the State, carefully catalogued and arranged. His special work seemed to be in the line of vegetable histology, though he devoted considerable time to embryology and bacteriology. He was enthusiastic, promoting scientific culture in his city, organizing the Dixon Biological Society and for some years continuing to be its zealous president. At the time of his death he had everything in fine condition for extended research along his favorite lines. He was highly esteemed in his city as a man of culture and Christian character. He was an earnest student of the Bible and prominent as a trustee of the First Methodist Episcopal Church in Dixon. In his death the Society loses one of its most valuable members.

PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MICROSCOPISTS.

MINUTES OF THE THIRTEENTH ANNUAL MEETING,

Held at Detroit, Michigan, August 12th, 13th, 14th, and 15th, 1890.[*]

TUESDAY, *August 12th, 1890.*

The Society was called to order by the President, Dr. Geo. E. Fell, in the High School building, promptly at 10 A. M. Thirty-nine persons were present at this time. Prayer was offered by the Rev. Dr. Moffett, of Washington, Pa., after which Hon. Stephen A. Griggs, acting mayor of Detroit, warmly welcomed the Society to the city. To this President Fell responded as follows:

He spoke of the meeting of the Society in Detroit some ten years ago and the pleasure which he, with others, had experienced at the hands of the hospitable people of Detroit at that time. After speaking of the evidences of progress Detroit had made in the last decade, he referred to the American Society of Microscopists as an organization not to be valued by the number of its members, but rather by the character of the work in which the Society was engaged. It was the representative body of Microscopists of America as the Royal Microscopical Society of England might be said to be of Europe. When it was considered that in a city like Buffalo, of some three hundred thousand inhabitants, there was not more than a dozen active workers with the microscope, it could not be expected that great numbers would be in attendance upon the meetings of the American Society. The long distances of America constitute quite a barrier to a large attendance. The

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[*] The report of discussion herewith included is made up by the Secretary from his long-hand notes and, though sometimes given in the form of the first person, must not be understood as the exact wording of the speakers. The latter have had no chance to revise them. They are, however, as correctly reported as it is possible to do in this way.—T. J. BURRILL.

Society now numbers about four hundred members, and about one-quarter of that number are usually present at the meetings. As will be evident upon examining the program of this meeting, there will be no time to waste. If every moment is utilized, all the work cannot be fully considered. In the proceedings which will present the papers in full, all the members will reap the advantages of the existence of the organization, which is now on a secure footing, in good financial condition, and with a constituency interested in the progress of the organization as an establishsd necessary addition to the permanent scientific organizations of America. It has done good work, is doing good work, and its future promises well indeed if one-half of the workers with the microscope in our vast territory will only support and interest themselves in the organization.

Hon. L. L. Barbour, chairman of the local committee, made announcements as to excursion and entertainment, and in a witty manner promised on the part of the local committee to do anything and to furnish anything members desired if they would let their wants be known.

On recommendation of the Executive Committee the following-named candidates for membership were then elected :

H. D. Geddings, Key West, Florida ; Albert T. Lytle, Buffalo, N. Y. ; A. Wilson Dods, Fredonia, N. Y. ; Walter Storrs Bigelow, Buffalo, N. Y. ; H. B. Murray, Tonawanda, N. Y. ; Chas. C. Faris, St. Louis, Mo. ; Frederick C. Leber, M. D., Louisville, Ky. ; Lawrence A. Harding, Fergus Falls, Minn. ; Henry H. Doubleday, Washington, D. C. ; Arthur F. Bartges, Akron, Ohio ; John Ashburton Cutter, M. D., New York city ; Augustus C. Gruhlke, Waterloo, Ind. ; Mason B. Thomas, Ithaca, N. Y. ; Willard W. Rowlee, Ithaca, N. Y. ; Wm. J. Gillette, M. D., Toledo, Ohio ; Thomas Kennedy, New Brighton, Penn. ; Pierre A. Fish, Ithaca, N. Y. ; Grant S. Hopkins, Ithaca, N. Y. ; Thomas B. Spence, Ithaca, N. Y. ; Frank A. Rogers, M. D., Brewster, Mass. ; J. P. Pfanner, Buffalo, N. Y. ; F. J. Schaufelberger, M. D., Hastings, Neb. ; Wm. H. Sylvester, Natick, Mass. ; Geo. H. McCausey, Janesville, Wis. ; Louis C. A. Last, Moberly, Mo. ; Robert Reyburn, M. D., Howard University, Washington, D. C. ; Thomas H. Urquhart, M. D., Hastings, Neb. ; James W. Hartigan, A. M., M. D., Morgantown, W. Va. ; Walter N. Sherman, M. D., Merced, Cal. ; James A. Close, M. D., Summerfield, Ill. ; Jose M. Yznaga, Washington, D. C. ; Wm. R. Chittick, M. D., Detroit, Mich. ; Frank Davis, Belleville, Ill. ; Henry M. Whelpley, St. Louis, Mo. ; Geo. W. Rice, Detroit, Mich.; Charles Willich, Jr., New York city; Lyman L. Deck, M. D., Salamanca, N. Y.; Edmund W. Weis, M. D., Ottawa, Illinois.

The Secretary announced the deaths of Dr. F. S. Newcomer, of Indianapolis, and of Engene Pinckney, Dixon, Illinois, and obituary notices were referred to the Committee on Publication.

Lyman Deck, M. D., read a paper upon the Resolution of Amphipleura by central light, in which he stated that he had clearly seen the transverse lines on balsam-mounted frustules by lamplight with mirror and condenser perfectly centrally fixed. This was accomplished with a $\frac{1}{12}$-inch homogeneous immerson objective by Gundlach.

Gov. J. D. Cox said he hoped that before the paper shall appear in the Proceedings Dr. Deck will add to it the measurement of the actual number of striæ in the specimens of Amphipleura used. This shell varies so much in this respect that statements of resolution of Amphipleura are of small value unless the striæ of the particular shell are counted.

The recent discussion of the subject by Dr. Van Heurck in Royal M. S. Journal also shows the importance of photographic evidence of what is seen, and the tendency in the scientific world is to be quite sceptical unless such evidence is furnished. It would, therefore, add very greatly to the value of the paper if Dr. Deck could give us this demonstrative proof.

Edward Bausch: Has Dr. Deck evidence that his illumination was truly central? We may remember the discussion some years ago between Dr. Moore, Dr. Detmers and others upon a resolution of Amphipleura with mirror in central position, but by looking down the tube the illumination was found to be decidedly oblique. The statement shows such a remarkable performance that I am inclined to believe it can be explained in this way, but this can be easily decided by looking down the tube.

Dr. John A. Miller: The Doctor did not state what eye-piece he used. This makes a great difference. With a solid $\frac{1}{4}$th-inch ocular *Amphipleura* in high medium mounting can be resolved with a Bausch & Lomb professional $\frac{1}{4}$th-inch objective.

Dr. Deck: I think the light used should be considered axial, though I did not look down the tube to test it in that way. In my work at home the light was admitted through a small aperture in the window shutter and received upon the center of the concave mirror, then passed through a diaphragm $\frac{1}{8}$th inch in diameter.

Edward Bausch: A slight swing of the mirror in such cases gives oblique illumination, and a yellow spectrum is obtained.

Dr. Deck: The mirror was very carefully placed and in every way obliquity of illumination was guarded against. The light was pure white. (See note, page 170.)

A paper upon the Microscope and Camera in the detection of forgery, was then read by Dr. Marshall D. Ewell, of Chicago.

In the discussion following, Henry L. Tolman said the laws now permit comparisons of different signatures. Each person has his own idiosyncracies. You observe it in his walk, in his talk, etc., as well as in his handwriting. There is variation in it all, but the variation is for each individual within certain recognizable limits. Hence it is essential to have as many elements in a comparison as possible. The defendant himself may be compelled to furnish numerous copies of his signature, or in other ways as many as practicable must be obtained, when an attentive study will bring out the peculiarities common to all. In the recent very celebrated forgery case in Minneapolis the defendant, while upon the witness stand was compelled to acknowledge 240 papers in his handwriting, and though at length the jury disagreed the case was practically won by the prosecution. In another case a suit for libel was brought, based upon anonymous letters, and in the course of the trial it was shown by comparison with papers which the defendant was compelled to recognize as his own that the anonymous letters were his also, and the case was so decided. Now in the case presented to us by the author of the paper there was certainly a very clever imitation of the signature. There is, however, a waver in the down strokes, and this is an excellent way of detecting the forgery if it can be shown that they do not occur in the original. Simple similarity of handwriting does not mean much, because all copy another at some time; but when the similarities include the peculiar, individual characteristics, then some direct relationship must be suspected. In this case the man's imitation of the lady's hand had been well done, but could be detected in the manner mentioned.

The next paper, by Grant S. Hopkins, of Ithaca, N. Y., upon the Structure of the stomach of Amia Calva, was read by the author.

Professor Gage stated that the work had been undertaken at his suggestion because Americans should especially apply themselves to the forms of life peculiar to this continent and not thrash over the old straw of European laboratories. In this case the author of the paper had spent a long time upon his subject, doing his work with much care, and the result was a contribution to knowledge in the line of American histology worth working for.

The President: What supposed function has this ciliated epithelium in the stomach of this animal?

Mr. Hopkins: That has not been determined.

Willard W. Rowlee then read a paper upon Imbedding and Sectioning mature seeds by the paraffin method.

Mr. Tolman observed that the method described was no doubt the only way to get good sections of whole seeds, but one could hardly be sure that the process had not changed the true appearance of the structures. He had found the best way was to select the material at just the right stage of growth or maturity, when there is a sufficient amount of mucilaginous substance to hold the parts together. He would then cut without previous manipulation.

Professor Gage: A considerable part of the work described was done in my laboratory, and so I had a chance to know about its results. Have been cautious about the use of paraffin with animal tissues and can appreciate the danger of change in tissues during the methods of preparation, but can say that there was no considerable amount of deterioration in the present case. Moreover, it is the only way to obtain serial sections and these are a necessity in numerous instances. Points can be made by their use which are impossible otherwise. To study whole seeds nothing but serial sections will suffice.

Mr. Rowlee: If it is true that good sections of such material can be made without imbedding, nothing further can be desirable, but I have thoroughly tried and failed; cannot get along without some method of imbedding.

The Secretary expressed his gratification in the presentation of the paper and his conviction of the importance of the process described to botanists.

Adjourned.

T. J. BURRILL, *Secretary*.

Afternoon Session.

When the President called the Society to order at 2 p. m. there were 36 persons present. This number was increased to about 60 during the afternoon.

The first item of business was a discussion opened by Professor Ewell, of Chicago, upon a proposed standing committee of the

Society on medico-legal microscopy. The following is a rather meager abstract of the discussion—all the Secretary was able at the time to report:

Dr. Ewell: In matters pertaining to the public welfare two things are most important, viz., the preservation of health and the preservation of life, and the microscope has much to do with both. I cannot but appreciate the high degree of excellence of the modern compound microscope, yet it is not a perfect instrument. Its powers are definitely limited and a strong protest must be made against the too prevalent practice of claiming too much for it. A recent communication in a prominent microscopical journal claimed facts in regard to the Cronin murder which should not be tolerated in respectable print. The statement was made that not only had blood found been identified as human blood, but that it was from the body of Dr. Cronin! A committee should be appointed to help prevent the dissemination of such monstrous untruths, even if editors do hide behind the common assertion that they are not responsible for the statements of authors. There should be some way of establishing a general standard by which all such matters could be measured and the ridiculously false separated from the true. This proposed committee should make such a standard.

Dr. F. L. James agreed with the last speaker in the evil resulting from praising the microscope beyond its capacity of performance. The bombastic claims made for the instrument did more harm to real progress in the direction in which this Society is especially interested than anything else. Such articles as that cited are sent to all journals. An editor is responsible for printing such things. In his own journal the statement was not printed in regard to the editor not being responsible for the views of correspondents. An article had recently come to him in which the writer claimed to have proved that a certain skin disease was due to the spicules of sponges, because he had cultivated the spicules and made successful inoculations therewith. To publish this would give indorsement to the absurd idea. So in regard to the papers presented to this Society. They should all pass through the hands of a competent committee before the titles were placed on the program.

Dr. W. H. Seaman: Would also second the movement for the committee suggested. It is one of the ways by which this Society can make its influence felt. We should agree upon a standand by which all general statements can be tried. In this way the efficiency of expert testimony can be augmented. The association of agricultural chemists have established a standard which is accepted in the courts, and there is no reason why this Society may not do the same for its own department of knowledge.

H. L. Tolman: It is doubtful if any action we might take would be considered worth anything in the legal sense. In a court everything must be presented by sworn witnesses or by other persons under oath. The thing, however, that can be done is to raise the public standard of information and so make all capable of discerning what is notoriously false from what may be accepted as possibly true.

Dr. Seaman: A committee could formulate matters so that others besides specialists could tell what might be expected and what could not be looked for as results of microscopical work. Let the facts be stated as to the identification of human blood and let this receive the sanction of this Society; it would certainly help before a jury to settle important cases.

Dr. J. O. Stillson: Courts of justice expect us to state confidently whether or not a specimen is human blood, human hair, &c. Now, there is enough that can be decided by a proper use of the microscope, but it is altogether bad to claim more. It should be understood in some way what ought to be expected and the testimony confined within proper limits. A committee ought to help do this.

Dr. Ewell: I have no faith in being able to produce much of an impression upon public sentiment. If we can improve the ideas and expressions of scientific men, we shall be doing well. In such matters every one should invite criticism from every seeker after truth. We must be scientific agnostics—believe nothing and know nothing except that capable of positive demonstration and not until proved beyond cavil. Criticism then can do no harm, and before this may do good.

Professor Claypole: Such a committee may provide a standard by which not only men of science may be guided, but by which lawyers and judges may inform themselves and govern their actions accordingly. A good help of this kind would tend to dispose of the farce too frequently presented when an astute lawyer pits the testimony of one expert against that of another.

Dr. Stillson: The work of the committee will serve as a check upon ourselves. We are very apt to be enthusiasts, and as such carried beyond proper footing. We pose in the newspapers and otherwise as authorities without having had sufficient study and experience in the special line of facts in hand. We want something to show that when it is claimed that by a microscopical examination of blood two persons are pronounced fit for marriage, or that an individual is fit for some certain occupation, that any one may know the claim is a humbug. The best workers in their own fields ought to be appealed to to set a standard of what can and what cannot be expected, and then this ought to be relied on as truth or the nearest possible approach to it. There is work for such a committee.

The President: Of how many persons shall the committee consist?

After some discussion on this point the following were appointed: M. D. Ewell, W. H. Seaman, H. L. Tolman, J. O. Stillson, and E. W. Claypole.

A paper upon the Application of Microscopy to Pharmacy, by Dr. H. M. Whelpley, of St. Louis, was read by title, in the absence of the author, and referred to the Committee on Publication.

The next paper, upon Microscope Objectives, was read by the author, T. J. Burrill.

Dr. J. A. Miller: If it is a fact, as has been asserted elsewhere, that the front lens of Zeiss apochromatic objectives is made of fluorspar instead of glass, the durability of the objectives must be seriously questioned, for this mineral is not reliable compared with well-known kinds of glass.

Professor Gage: Have used the apochromatics ever since their introduction with the very best satisfaction. Thought an 8 mm. objective had deteriorated. Looked as though there were ashes on the back lens—not dust, but ashes. Showed it to Dr. Ewell, and he thought it must be due to the corrosion of the lens.

H. L. Tolman: The paper detailed results obtained in a careful way and as such was a model presentation, but there is perhaps the liability of a misunderstanding in regard to the value of wide angles. Abbé's researches go to show that future advance is likely to be made with central light and with wide enough aperture to pick up the rays necessary for the best pictures. We have learned that in these matters we must be guided by theory as well as by experience and practice. In case of difference in results we must trust the wide angles in preference to the lower ones. In regard to Gundlach's objectives, it is nothing new, neither is it any disparagement to say, that they vary much in quality. The best are superb, almost or quite beyond competition, but others of the same nominal grade are much less valuable.

Dr. Ewell: My work has, to be sure, been principally upon ruled lines, but I have long since found that I wanted just as wide-angled objectives as I could pay for. The wider the aperture the better the results; have been abundantly satisfied with those of the Bausch & Lomb Optical Co. In the study of blood I have preferably used a $\frac{1}{10}$th homogeneous immersion of Spencer, using an amplifier when necessary with it. This has seemed to me equal to Zeiss' apochromatic, except upon *Amphipleura* with central light.

Professor Gage: The author of the paper states that he chooses for students' use a $\frac{1}{2}$-inch and a $\frac{1}{5}$th-inch objective. I prefer at least a $\frac{1}{8}$th inch. The $\frac{1}{5}$th inch does not give sufficient magnification. Students often fail to recognize the objects seen under such a power from the figures in the books, when they would do so readily with greater magnification.

Professor Kellicott: Can Professor Gage suggest an $\frac{1}{8}$th that he would recommend in place of the $\frac{1}{5}$th?

Professor Gage: Any good, dry, non-adjustable objective of that power will do.

Governor Cox: We are in danger here of putting the matter under discussion upon an incorrect footing. Instead of speaking of the focal length, the angle of aperture should be substituted. I have for some time been in the habit of selecting indifferently the objective I wish to use upon a given object by the angle and of making up the mere magnification necessary by the eye-piece. I now have in my possession a $\frac{1}{4}$th inch that will do anything that any $\frac{1}{10}$th inch of the same angle will do. The difficulty, however, comes in the manufacture of the wider angles of low power. The opticians can furnish us better goods for the money when the focal length corresponds in some degree with the angle of aperture.

Professor Gage: I can certainly agree with what Governor Cox has just said. It is, however, necessary in most cases to pay special heed to the item of cost, and, taking this into account, I have found an $\frac{1}{8}$th-inch dry objective of good angle the most satisfactory and economical.

The next paper, Additional notes on Gomphogaster, by C. M. Vorce, was read by title and referred to the Committee on Publication.

Professor Gage then read a paper prepared by Dr. Wm. C. Krauss, of Buffalo, N. Y., on Some methods of treating Nerve Tissue.

Dr. Ewell proceeded with a review of some of the medico-legal questions involved in the Cronin case.

Mr. H. L. Tolman, of Chicago, said that in discussing the subject he should try to avoid entirely the personal element and confine himself strictly to facts. It was greatly to be regretted that so many and so gross mistakes were made in the newspaper reports, and, worse still, in the stenographer's original record. No opportunity could be had to correct the stenographer's notes, and he was made to say things very different from the real testimony. This latter he still adhered to and wished it might be exactly known; should not fear criticism.

It was very unfortunate that the experts called in the case had so little time after the material was presented to them before they were called on to give their testimony in the court. The public excitement was so high that many things were managed differently from what would have been the case under other circumstances. Dr. Belfield and himself were engaged by the prosecution. They had been furnished with a chip of wood, taken from the floor of the Carlston cottage, on which were two drops of blood, and with some blood-stained cotton. To hasten the examination these materials were divided, Dr. B. taking the cotton and himself the bloody chip. From this last only 56 perfectly round normal blood corpuscles could be obtained. Had very carefully removed the blood to get it all. Would have been glad to have much more of it, but basing his information upon these and on the presence in the clot of several specimens of lanugo, he testified that the strong presumption was that it was human blood. The blood corpuscles averaged 1–3310 of an inch, considerably above the average. Did not say positively the blood was that of a human being. In dissolving the stain he had used a salt solution of 1.055 specific gravity, the specific gravity of the whole blood instead of that of the serum, because he was anxious that the error, if any, should be upon the right side, on the side of human life, with which his testimony must be weighed. The blood corpuscles are subject to the rigorous laws of osmosis, and as the normal serum has a specific gravity of 1.028 the salt solution used could not make the corpuscles too large. As to the exclusion of certain elements, this too was done with full knowledge of the requirements to obtain accurate results. No one knows what the so-called blood-plates are, either in their origin or destiny.

Everything which did not fall within the wide limits of 1–5000 to 1–3000 of an inch was excluded because they could not be known to be blood.

Now in regard to lanugo. According to the theory of evolution and descent now so commonly accepted, the hair remnants on the human body are degraded in size and structure from original, fully developed hairs. Such degraded elements have certain characteristics different from normal forms. They must be studied as degraded forms if we would arrive at truth, not compared with normal structures. It is so with parasitic plants and with parasitic animals, both of which are mostly much reduced in structure from that of the types. He had, therefore, taken every possible opportunity to collect lanugo; had secured large numbers of samples from patients at the Cook County hospital, and had collected from every animal in Barnum's menagerie as well as others, and after much study had failed to find any identical with that on the human body.

Now, as there are only a few grains in weight of this substance on the whole human body and as it is comparatively permanent, there cannot be much of it floating in the air or to be found hap-hazard on any other surfaces. The reported reliance placed upon epithelium scales was a mistake. Nothing decisive was claimed for the presence or condition of this in the blood-clot.

In regard to the measurement of blood, it is not hard to show what should be taken for the true periphery of the corpuscle. This is lens-like in shape and effect upon light. The dark boundary line results from a dispersion of the rays, and as this is nearly equal in both directions the center of the dark border must be the true edge of the corpuscle. Used both Rogers' ruled glass and filar micrometer with essential agreement in the measures, but prefers the former for this purpose. Had given much attention to the effects of disease on the size of the corpuscles. As a rule, in anæmic diseases the corpuscles are less and in inflammatory diseases they are larger than the normal dimensions. Embryos have larger nucleated corpuscles, but post-embryonic corpuscles soon become normal size.

E. H. Griffith: Does the habitual use of alcoholic stimulants change the size or character of the blood corpuscles, or does sudden excitement make any difference?

Mr. Tolman: There is no change in the blood corpuscles from sudden excitement, but there, no doubt, is some modification due to stimulants. It is found that there are proportionally more small corpuscles after meals than at other times.

Prof. J. A. Miller: In connection with this question of blood, it may be interesting to state that an examination of the blood, of

Kemmler showed that it had an acid reaction, but no other change from the normal condition could be detected, either microscopically or by the spectroscope.

E. H. Griffith: Mr. President, may I inquire of the gentlemen present whether any of them have any knowledge of the effect upon the blood of long-continued alcoholism?

Dr. McIntosh: I can say that the blood is considerably affected by excessive alcoholism. I once examined the blood of one of the worst cases of the kind I ever knew. Incredible as it may seem, the man actually swallowed three to four pints of strong Bourbon whiskey daily. The blood corpuscles were certainly much smaller than usual, and one-third to one-half of them were much crenated and otherwise abnormal.

Dr. Ewell: I should just like to say that the rule was entered that all expert witnesses in the Cronin case were to be examined separately. I had nothing to go by except the record as made by the stenographer and the newspaper reports. In this so-called restoration of blood it seems to me there are too many loose statements. How can blood-plates be distinguished from blood-clots? Know of no way to restore blood-plates.

The President: Dr. McIntosh will show with his projecting lantern to-morrow blood taken from Kemmler, both from the leg and from the brow, and it will be seen that there is a marked difference between the two. Due notice will be given of the time, so that all may be present.

Professor Kellicott read by title his paper on the Rotifera of Central Michigan, after which Professor Gage presented a paper upon the Transition from Columnar to Stratified Epithelium.

Adjourned to meet informally and socially at the hotel headquarters during the evening.

T. J. BURRILL, *Secretary.*

WEDNESDAY, *August 13, 1890.*

The meeting was called to order at 10 o'clock A. M. by President Fell, at which time 47 persons were present. This number subsequently increased to 60.

On recommendation of the executive committe the following-named persons were elected to membership: H. O. Walker, M. D., Detroit, Mich.; R. C. Olin, M. D., Detroit, Mich.; Stuart H. Perry, Pontiac, Mich.; R. C. Hutton, M. D., Detroit, Mich.; James Cleveland, Jr., M. D., Detroit, Mich.; Oscar Edward Cartwright, Detroit, Mich.; Clarence D. McLouth, Ypsilanti, Mich.; John Bridge, Detroit, Mich.; W. G. Latham, Detroit, Mich.; H. C. Wyman, M. D., Detroit, Mich.; A.W. Imrie, M. D., Detroit, Mich.; W. R. McLaren, M. D., Detroit, Mich.; J. B. Book, M. D., Detroit, Mich.; Mary E. Van Schoonhoven, Ann Arbor, Mich.; Dr. Anson S. Traser, Sarnia, Ontario; T. L. Hazzard, M. D., Allegheny, Pa.; A. E. Hiss, Chicago, Ill.; Herbert Walker, Pittsburgh, Pa.; Charles E. Barr, Albion, Mich.

A report of the committee appointed yesterday on medico-legal matters was accepted and referred to the Executive Committee to place it upon the program at some suitable time for discussion.

After an announcement by Dr. Manton in regard to a working session and remarks by several members, it was moved and carried that a working session be arranged for, beginning at 2 P. M. Thursday, the 14th August, and that at 3.30 P. M. the same day a session for the reading of papers be held. It was then ordered by vote that we meet at 9 o'clock, local time, Thursday morning.

The first paper of the morning session was by Dr. M. D. Ewell upon "Some experiments to determine the limit of vision as related to the size of the object observed." Nineteen different persons tried to determine at what distance one square mm. of black color on a white background, or the reverse, could be just seen and at what distance it could be just defined as to form. The experiments were tried in a well-lighted room in the daytime and by persons from 17 to about 50 years of age. The average distance for the former proved to be 26 feet 4 inches for the black square, and 22 feet 11 inches for the white square, and for definition of shape in the two cases the mean results were respectively 5 feet 10 inches and 5 feet 7 inches. Reasoning now upon the visual angle subtended, the author deduced the conclusion that at a distance of ten inches a black square on a white ground must be 1–790 inch on a side, and that with an amplifying power of 1,000 diameters and a tube length of 10 inches, a similar object to be visible must be at least 1–790,000 of an inch on a side, while to be defined these dimensions must be respectively 1–175 of an inch and 1–175,000 inch. Experimenting in the same way with himself, the author found that to be seen at ten inches the size must be 1–870 inch, and to be defined 1–155 inch. Much smaller *lines* can be seen. It is quite likely that different colors would give different results.

The possibility of error on account of the difference between macroscopic and microscopic vision was alluded to, but it was thought that the errors here would be no greater than would be liable to occur in other methods of determining this limit of normal vision. It is expected that the author will, at a future time, report the results of further experiments of this kind.

In regard to the measurement of minute objects, Dr. Ewell stated that with very high powers, such as 1–25-inch objectives, the filar and Jackson micrometers were practically equal; but for moderate magnification the former is far ahead of the ruled glass.

Dr. Seaman: We are very liable to make mistakes when we use the results of experience with unaided eyesight for determinations of microscopic vision. It is well known that lines are easier to make out than dots, yet many persons whose eyesight is supposed to be normal cannot count ruled lines 1–100th of an inch apart. The American Society of Photographers have been trying to determine the conditions of a fixed focus for use with objects at all distances, and it had been determined that lines 1–250th of an inch wide could not be distinguished by an ordinary individual from those of less dimensions, and so blur to that extent would not be recognized. Training in such matters has much to do with results, but probably 1–250th inch is less than anybody would be likely to measure with his unaided eye.

Dr. Ewell: Did you say that one second of arc was visible?

Dr. Seaman: It is so stated in text books.

Dr. Ewell: So much the worse for the text book.

Governor Cox: We must keep in mind that in microscopic vision the wave length of light counts as a factor, and so we must be careful about our deductions when we reason from natural eyesight to that of microscopic vision.

Gov. J. D. Cox now opened the discussion upon representation of the Society at the World's Fair in Chicago, 1893. He said the opportunity for the assemblage of juries of competent experts in all lines of science and art, as well as in other matters, would be very great, and great inducements to attend will be offered. There will undoubtedly be a great display of instruments and apparatus, and so there will be congregated a great number of workers who as specialists make use of the various devices. There ought to be, and doubtless will be, gathered the largest body of such workers ever brought together. The question now is, how shall the representation be made and who shall manage it? This organization will be looked to for advice and help. Some one must study the problem, must acquaint themselves thoroughly with what has heretofore been done, and must recognize what is now demanded, and devise methods for procedure. Shall we have a special committee appointed for this purpose, or shall the subject be left with the officers of the Society? It so happens that the Secretary to be chosen at this meeting will hold his office until after the work of organizing and inaugurating the fair is completed, and attention to this point ought to be considered in the election. The Secretary is, presumably at least, the working man among our officers, and upon him will devolve much of the responsibility of the occasion. Again, what can our members resident in Chicago do? It might be an excellent thing if a Secretary can be selected from one of their number.

The fears expressed that the nations of the earth will be deterred by our restrictions upon trade, etc., from making a great display are, I trust, without foundation. There is no reasonable need for fear in this direction. It is a foregone conclusion that there will be a great exhibition. We are not to consider whether or not we shall aid in this, but must decide what we shall endeavor to do and then select the men to carry forward the work.

H. L. Tolman: The State microscopical society, with its headquarters at Chicago, has had the matter under consideration and asked that its delegates to this convention look after the subject here. Upon this authority he now extended to all members of the American Society the heartiest invitation to attend the fair and to take part in whatever may be agreed upon for the exhibition and meeting. The thought had been advanced of combining with photographic and other optical affairs, but it seemed to be the opinion that the microscopical exhibits and interests should be kept separate. It is proposed to have an appropriate place designated as headquarters, where all workers with the microscope will find a cordial welcome. The directors of the fair will favor anything and everything of this kind, though no doubt some of the expenses will have to be borne by those who take part. The local board cannot do more now than offer space, but it is assured that this will be furnished in any reasonable amount. It is believed that Congress will permit the exhibition of the material in the National Museum and in that of the Army and Medical Museum, including the unrivaled work of Dr. Woodward.

It is eminently proper that this Society should lead off in this great undertaking; and, if so, rest assured the Illinois State Society will fall into line and work with a will.

Dr. L. D. McIntosh: It has been proposed to induce the local board to put at the disposal of such organizations a suitably equipped hall for meetings, lectures, demonstrations, &c., in connection with a display of instruments. He felt like personally assuming the responsibility of furnishing a large amount of apparatus for such purposes and would lose no time when he returns home in endeavoring to start the ball rolling.

Dr. Seaman moved that a committee be appointed by the Chair to consider the matter and to take such action as may be deemed necessary and expedient. The following-named persons were so appointed: J. D. Cox, W. H. Seaman, L. D. McIntosh, H. L. Tolman, J. A. Miller. This committee was empowered to add others to their number if considered desirable.

Dr. A. Clifford Mercer then presented his paper on "A mooted matter in the ordinary use of an eye-piece in photo-micrography."

In the discussion following Governor Cox said that the compound microscope was an instrument composed of an objective in one end of a tube and of an eye-piece in the other end of this tube. Thus constituted, the instrument was one thing, and the removal of any of its parts destroyed the instrument in an essential and critical sense. If any part is made use of by itself you are working hap-hazard and are exceedingly liable to get poor results; had once asked Mr. Spencer about the correction of his objectives and had been informed that they were adjusted by the use of a certain eye-piece. Hence, to get the best results with that objective it should be used with the identical eye-piece with which the optician himself made his corrections, or at least with one of the same construction. In his own practice he had for a number of years used the same eye-piece in photographic work that he used for direct vision with his collar adjustments the same, and, on account of the nature of the objects photographed, the reproductions had gained wide repute and had been examined by many persons. Dr. Detmers had also done some remarkable work in the same manner, though with oblique instead of central illumination. His photographs of Amphipleura were equal to any that had been examined by the speaker. We may therefore say that at least as good results have been obtained with the use of the eye-piece as in any other way, and as to the theory of the instrument we are liable to the most surprising misapprehensions. What, indeed, do we know of the optical properties of a compound microscope? We treat the parts as though they were simple lenses, and then from them endeavor to make up our theory of the combined instrument, and no one can know that his conclusions are correct without practical demonstrations.

Now it is of course recognized by all that the adjustment of the objective must be properly made in correlation with the distance of the conjugate focus. This last may vary greatly. Dr. Woodward used the whole room as a tube for his instrument. If we are told that in changing the distance of the conjugate focus we spoil the instrument, a great deal is assumed. The disturbance is likely to be much less than it will be by inserting any other optical contrivance, such as an amplifier, not considered in the original correction of the objective. This is well shown by the extensive and critical trials of numerous pieces by Dr. Woodward, during which only one amplifier was found to work satisfactorily. Of course the focus must be readjusted for photographic work and the results obtained in the paper are just what theory and experience demands.

Secretary Burrill: I need not say to Dr. Mercer that I have been greatly interested in his account of the experiments tried and in the results obtained, because I was one of the members alluded to in the paper who had expressed my inability to explain the apparent phenomena observed by myself. The matter has been during several years a veritable puzzle to me. I would gladly accept the explanations made, because they are to my mind just what theory says they must be, yet the stubborn fact remains that by placing a screen at a given relative distance from the eye-piece I can make the sharpest possible photograph without the least adjustment of the focus from that procured by the eye. Now, we agree theoretically that the rays from a given point of an object under the microscope emerge from the eye-lens *diverging*, and that traced forward these diverging rays (converging forward) form the virtual image at a point where they apparently meet or would meet if prolonged forward from the eye; but such diverging rays can never, according to theory, again form a picture upon a screen or anything else until they are made to converge and form an image where they meet. Now this is just the impossible thing that I have done unnumbered times and can readily do over again with certainty of success. With my photo-micrographic apparatus, a home-made affair, I can remove the connection between microscope and dry-plate holder, insert my head and adjust the focus after the ordinary fashion by the eye applied at the ocular. Without moving the instrument in any way the connection with screen is readjusted and exposure made with a sharply defined picture as the result. This is the dilemma in which I find myself: Theoretically I know it cannot be done; practically I know it can be done.

Dr. Mercer: Are your eyes presbyopic?

Secretary Burrill: Yes, sir; I use convex eye-glasses.

Dr. Mercer: Do you use your glasses when adjusting the focus of your microscope?

Secretary Burrill: No, sir.

Dr. Mercer: That is the explanation of the whole thing. In your special case the rays from a point of the object emerge from the eye-piece converging and must be so to suit your eye; hence the image on the screen is correctly formed.

Secretary Burrill: I shall be delighted to find that this explanation is the true one, for it is exactly what optical science indicates and demands.

Dr. McIntosh: There can be no question as to the accuracy of Dr. Mercer's results. The focus of the objective must always be adjusted according to the distance of the screen.

The paper was further discussed by Messrs. Tolman, Barr, Cox, and Claypole—all practically agreeing with the author's explanations.

Governor Cox said that he had tried all the prominent kinds of eye-pieces and had sometimes used the screen three to four feet distant, but had never found anything better than the Huyghenian eye-piece.

In answer to Professor Claypole, Mr. Tolman said that in good objectives the actinic and visual foci were practically the same with higher powers than a $\frac{1}{4}$th inch.

Dr. Mercer could not agree with Governor Cox in holding that as the objective is always corrected with an eye-piece, never without, thus making the whole compound microscope a unit in action, therefore an eye-piece should always be used with the objective to get the best results in photomicrography. He called attention to the fact that in the ordinary use of the microscope a virtual image is seen; and, further, that the whole instrument is corrected to make that virtual image as perfect as possible for rays of light taking mathematically defined paths through the various lenses. In photomicrography with the whole instrument, not a virtual but a real image is formed by rays taking not the same but very different paths through both the objective and eye-piece. Now, for example, the rays incident on the eye-lens come from an aerial object beyond its principal focus, while in the first instance rays are from an aerial object within the principal focus of that lens. The eye-lens is not achromatic, but in the first instance its aberrations are corrected by its relative position. In the second instance the radical change in the paths of the rays allows both chromatic and spherical aberrations to vitiate still farther the already faulty image formed by the objective working at the disadvantage of a change in position (at a greater distance from the object than that for which it is so nicely corrected). It is an error (*The Microscope*, Naegeli and Schwendener, London, 1887, p. 161) to suppose the Huyghenian eye-piece compensates for residual aberrations of the objective. The corrections of the objective are complete in themselves and are especially such as enable the objective to project a real image at a certain distance. This, then, is the best image to which the photomicrographer can expose his sensitive plate. If this image is too small it can be enlarged by projecting it to a greater distance by the use of a specially corrected negative lens, as was practiced by that best of workers in photomicrography, Dr. Woodward, or an enlarged image of the image can be projected by specially corrected lenses after the Abbe method. A Huyghenian eye-piece is not corrected for this, but for a radically different purpose, and is, therefore, at least theoretically, quite out of place as a projecting lens.

The next paper, by Dr. R. N. Reynolds, on Making gelatine and injecting small animals, was read by himself.

Following this came a paper by Professor Gage on "Picric and chromic acid for the rapid penetration of tissues for classes in histology." At the conclusion the author said that the main value claimed for the method described was that students might thus prepare their own material, which he believes to be a matter of importance, and asked for the views of other instructors.

Messrs. James, Tolman, Claypole, and Mercer replied, all heartily in accord with the speaker.

Professor Claypole said that sometimes students were at first inclined to rebel, thought they were not learning anything, but after a time gained much greater enthusiasm and inclination to go on than by the cramming process, by which they, perhaps, obtained more facts, with much less likelihood of making use of their work in after life.

Dr. Mercer thought it a good plan to get students to buy microscopes and work with their own instruments, taking more interest in consequence and being much more likely to continue their studies beyond the time of school attendance.

In the absence of Dr. Lucien Howe, whose paper on "The action of bacteria on the conjunctiva of the rabbit" was announced upon the program, Dr. F. L. James called attention to the proper distribution of the printed proceedings. He said the Society was not known according to its merits. He had written several well-known journals requesting the publication of the announcements of the meeting, etc., and had word returned that the Society had never been heard of. Others had willingly inserted such matter. He moved that the proper officer be authorized to distribute from 100 to 200 copies of the proceedings where they will do the most good in advertising the Society. The books are too valuable to be buried.

President Fell: This is a very important matter. The Society ought to be much better known than it is. When making arrangements for the meeting here in Detroit persons approached knew nothing of the association or of its work. The medical journals were favorable to the work of the Society when they knew what was done, and some of them had published his first circular in full.

Mr. Tolman seconded the motion and hoped that the printed proceedings would be widely distributed.

Governor Cox said there was danger of not knowing just what we are doing. Many new members will want sets of the proceedings, and it is probable that the earlier numbers will be hard to furnish. We all know how this is with many European societies, whose earlier transactions are now scarce and esteemed of high value.

Professor Gage wished to emphasize this last statement. He would hold carefully the back volumes, but would liberally send out current numbers. The publications are worthy ones and would do us honor in the great libraries of the country, yet very few of them have copies. These should be sent to all of the great journals interested in the subjects here presented, and they would help to put the Society in the position it should occupy in public knowledge and esteem.

Mr. Mellor made a statement concerning the amount of receipts from the sale of copies of the proceedings, after which Dr. Mercer moved to refer the matter to the Committe on Publication, to report at the next meeting, but to have power to act for the coming year. He also suggested that it would be well for the same committee to take into consideration a place of deposit for the duplicate volumes. His motion prevailed.

Professor Kellicott remarked that the Secretary has a list which had been approved by a former executive committee, and that this list should have been amended as necessary and the copies promptly sent out.

Adjourned.

T. J. BURRILL, *Secretary.*

AFTERNOON SESSION, *August 13, 1890.*

Society called to order by President Fell at 2 o'clock.

On recommendation of the Executive Committee, J. E. Baker, M. D., Wyoming, Ohio, and Edwin A. Strong, Ypsilanti, Mich., were elected members.

The first subject presented was by Professor Gage on Uniformity of tube length, after which Edward Bausch read a paper on "The full utilization of the capacity of the microscope, and means of obtaining the same."

The discussion which followed covered both subjects.

Governor Cox: There is a method of determining the length of the conjugate focus not directly referred to by Professor Gage, which is worth keeping in mind in this discussion. Reference is made to the so-called Cross formula with Dr. Woodward's modification of it. The first was originally published in Silliman's Journal. As a basis for this formula the distance from the object itself to the image as formed in the Huyghenian eye-piece is measured. This image, as is well known, is situated in the diaphragm of the eye-piece. This measurement does away with the trouble arising from the variable length of the brass setting of the objective and with the position of the ocular in the tube. It also gives us a scientific value not vitiated by any mechanical variations.

There is another thing that has been generally overlooked and is of unrecognized importance, viz., the difference in amplifying power by the objective with the adjustment collar in different positions. In some cases the difference amounts to nearly one-half the total amplifying power, an objective magnifying 500 diameters when open giving 750 when closed. He had made a table giving the magnification for each number on the adjustment collar and found it of considerable service in various ways.

Mr. Tolman: There is still another point from which tube length may be counted, viz., the posterior conjugate focus for parallel rays. This lies commonly from one-half to three-fourths of an inch outside of the objective, but in higher powers considerably further forward. The determination of this point is not so simple a matter as measuring the brass-work, but it may be worth the while to take more trouble to secure better results. As to the upper point, it is no doubt best to take the upper end of the draw tube and then make all oculars so that the image will correspond with this. There would be trouble sometimes in securing the last, as with binoculars, but nothing better can probably be suggested.

Governor Cox asked Mr. Bausch whether there is any reason for adopting any certain length for the tube as opposed to any other length.

Mr. Bausch: No; there is not. It is only necessary that some standard length be made, and then objectives can be corrected accordingly. Would be glad to follow the suggestions of this Society in this regard. He also afterwards stated that there did not seem to be any optical preference for the ten-inch tube over that in common use on the continent, unless the longer tube gave better facilities for correcting spherical aberration.

After further discussion by Messrs. Tolman, Mercer, Gage, Close, Ewell, and Burrill, on motion of Dr. Mercer a committee, consisting of S. H. Gage, Edward Bausch, and A. C. Mercer, was appointed to report, before the close of the meeting, recommendations for adoption by the Society upon the upper and lower points between which tube length shall be measured.

Professor Gage then exhibited for inspection and criticism some diagrams of the microscope designed for aid in instruction, and also a modification of Abbé's camera lucida. The latter improvement consisted in a graduated arc by which the proper angle of the reflector can be readily adjusted. He commended this camera as the best in use and said that anybody could use it with much satisfaction after a very little practice. Several points brought out very clearly by showing the instrument itself cannot be readily reported here. It was, however, generally agreed that the camera was an exceedingly valuable aid in drawing, surpassing for this purpose any other known.

A paper upon the Ammoniacal fermentation of Urine, by Dr. V. A. Moore, was read by Dr. Seaman.

The next paper, by Pierre A. Fish, on the Epithelium of Brain cavities, was read by Professor Gage, after which a paper was presented by Professor and Mrs. Claypole, called Notes on *Baryeidamia parasitica, Karst.*, found on stored onions. The last was really a description of a parasitic mould upon other moulds found on onions and elsewhere. The life history of the parasite was given as far as traced, and slides illustrating it were afterwards shown. It is believed to be the first mention made of the curious fungus in this country. The work had been principally done by Mrs. Claypole.

Professor Claypole then presented a resolution providing for associate memberships in the Society, which was referred to the Executive Committee for recommendation.

Adjourned.

T. J. BURRILL, *Secretary.*

EVENING SESSION, *August 13th, 1890.*

The Society convened, in its room at the High School building, at 8 o'clock, to listen to the President's annual address. At the appointed hour, a good audience being present, President Fell called Vice-President Seaman to the chair and proceeded with his address upon the Influence of Electricity on Protoplasm.

At the conclusion of the address the Society adjourned.

T. J. BURRILL, *Secretary.*

THURSDAY, *August 14th, 1890.*

The first thing of the forenoon session was a report on micrometry by Professor M. D. Ewell, who consented at the time to open the discussion in place of Professor Rogers, not present.

Dr. Ewell explained, by an illustrative example, the method of determining the error, if any, in micrometric ruling when the correction of the total length of the scale (as of a centimeter) is known.

This explanation the President thought worthy of permanent record and asked the Doctor to furnish the manuscript for the purpose. Finding it impossible to comply in time for this publication, Dr. Ewell promises a paper upon the subject for next year.

Professor Miller : Do you accept the scale of the filar micrometer as the maker furnishes it, or do you determine its value ?

Dr. Ewell: I have a filar micrometer by Zentmeyer and one by Bullock and both are works of art; have tested them with great care and find that the error in each case is less than the personal error in making the comparisons.

In answer to questions by Messrs. Seaman and Tolman the Doctor replied that a large number of observations should be made and the results averaged, and that all the spaces should be included unless there is reason to suppose some one or ones of them are incorrect. In measuring, his practice is to read from the left side of one to the left side of the next, counting in each case the edge in same way, sometimes leaving a line of light, sometimes just shutting this off; often puts finger on side of tube and pushes just enough to try the correctness of the position. If the milled head should be turned too fast turn a little way back and carefully approach position again, always turning so as to come up to the line in same way; uses a cobweb, not a wire. It is specially important that the head of the micrometer should turn very easily and smoothly.

The next subject upon the program for discussion was the advisability of adding more members to the Publication Committee, opened by Professor Kellicott.

He said that as the committee was now constituted the President-elect was made a member of this committee, though he had taken no official part in the proceedings of the meeting and could not be specially acquainted with the matter presented for publication. On the other hand, the retiring President, not now a member of the committee, had during the preparations for and the progress of the meeting every facility for gaining information concerning these matters. It therefore seemed wise to so change our law that the President of the Society should be a member of the Committee on Publication during the time when the proceedings of the meeting over which he presided were under consideration. He would further suggest that the two Vice-Presidents be added to the committee. The duties of these officers in the usual run of things were simply to audit the accounts of the Treasurer. This would give them something more to do and introduce them somewhat to the affairs of the Society, as well as secure their aid in the discharge of important business. The proper selection for publication and judicious management of this latter work must always be exceedingly important to the best interests of the Society, and the addition of these officers to the committee would be a help in that direction.

Governor Cox: In practice how do you succeed in getting effective consultation by members of the committee and thus relieve the Secretary of taking the responsibility upon himself?

Professor Kellicott: That is the difficult thing to do, because the members of the committee are liable to be widely scattered, but the papers offered for publication might be distributed among the members of the committee, assigning to each such subjects as he is best informed upon, and in this way gain better results than can be secured by the act of one man.

President Fell: This matter seems to demand changes in our constitution and by-laws. It would be very desirable for the committee on this subject, from whom we are expecting a report, to consult with Professor Kellicott and to embody in the new constitution these improvements suggested by one who is certainly better qualified, by his long experience and study of the subject, to make them than any one else among our number.

After some explanations concerning the failure of the Committee on Constitution to report at this meeting, upon motion of Governor Cox, Professor Kellicott was asked to formulate the suggestions made by him into by-laws, which could be adopted at this meeting and which could then be included in the general report on constitution next year.

On motion, the Committee on Constitution was given further time and to report at the next meeting.

After free nominations, the following-named persons were elected members of the Committee on Nomination of Officers for ensuing year: D. S. Kellicott, W. P. Manton, D. L. McIntosh, C. G. Milner, W. H. Seaman, G. S. Woolman, and C. C. Mellor. Subsequently S. H. Gage was selected to take the place of W. H. Seaman, who withdrew from the committee.

At eleven o'clock a recess was taken to witness some lantern exhibits by Dr. McIntosh in an adjoining room. The projections upon the screen were magnificently shown and included, among others, blood taken from the leg and forehead of Kemmler, executed by electricity. While that from the leg showed little or no difference from the normal appearance, that from the brow was manifestly and considerably altered, presumably by the electric current. An injected tooth of a dog showed a remarkable development of blood vessels and was a beautiful as well as instructive object. A very good photograph of a man was shown, taken with the crystalline lens from the eye of a calf.

Returning to the assembly room, Governor J. D. Cox gave, in an oral way, with illustrations on the blackboard, the substance of his papers on abnormal forms in diatoms, and a review of some of the generic and specific distinctions in the family Coscinodisceæ.

W. W. Rowlee: Is it possible to observe whether or not these divergent forms are absolutely connected in lines of descent from parent to offspring.

Governor Cox: Only to a very limited extent. We are obliged, at least in case of the free species, to draw our conclusions from the general association of the individuals, and such facts as can be made out in different ways. However, in the fasciculated forms better evidence can, of course, be obtained.

Professor Gage remarked that it was exceedingly refreshing to hear this discussion upon the change in form and characteristics within the limits of true specific distinctions, for much the same thing had been observed in regard to animals. He referred to a salamander, which had previously been described as two species, the one living upon the land the other in the water. The first had ciliated epithelium, the other stratified epithelium in its mouth; but instead of marking a distinction in kind, the change of color, structure, &c., was in some way correlated with the environment. He warmly commended the papers presented.

Professor Kellicott followed in similar vein, referring especially to infusoria. Had himself perpetrated species in a few instances upon insufficient knowledge of the possible variations within specific lines. Infusoria vary comparatively little in size, yet immense individuals may come from a cyst after conjugation.

Governor Cox: Among the diatoms the new, large individuals, the product of a conjugated pair, are the typical forms. As, after many generations, they become perceptibly smaller, they also become irregular and abnormal.

Professor Claypole: The subject as presented has been exceedingly interesting. Had long thought that a good deal of reduction ought to be made of the published species of diatoms, but had not anticipated that 10,000 such descriptions would or could be condensed to 1,000. Should like to inquire if it is possible to trace any recognizable variation according to geographical or geological distribution. At least among fresh-water species it would appear that a fine opportunity to study such changes might be found in the diatoms.

Governor Cox: As a partial answer it may be stated that in the fossil beds of these organisms there are very often noticeable characteristics and pecularities well marked for each bed. There is at least one difference, whether or not we consider these divergent forms distinct species or simply as varieties. It is much easier for beginners and students to gain a general knowledge of the group if the latter idea prevails. The introduction to the study of diatoms would be much simplified by admitting considerable variation within the specific limits.

Adjourned.

T. J. BURRILL, *Secretary.*

Afternoon Session.

At 2 o'clock the members assembled to take part in or witness the operations of the "working session." Strenuous endeavors had been made by the President and by Dr. Manton, of Detroit, to get the work of this session well organized before the assembly of the convention, but the result proved unsatisfactory, and it was left until members arrived in the city to complete the arrangements. In this Dr. Manton took charge and with some efficient assistance succeeded in making an excellent program, which was energetically carried out by the participants and enjoyed by the spectators.

Following is a complete list of the tables and operators, together with the class of work done by each:

Table No. 1—E. H. Griffith, A. M., F. R. M. S., Fairport, N. Y., miscellaneous work.

Table No. 2—Prof. Heneage Gibbes, University of Michigan, Ann Arbor, hyaline-fibroid degeneration, flagella on Spirillum.

Table No. 3—Prof. Simon H. Gage, B. S., Cornell University, Ithaca, N. Y., Winkel's marking apparatus for locating minute objects in a preparation.

Table No. 4—M. D. Ewell, M. D., LL. D., Chicago, Ill., micrometers and micrometry.

Table No. 5—Mary A. Spink, M. D., Indianapolis, Ind., sectioning and mounting.

Tables Nos. 6 and 7—W. W. Rowlee, Cornell University, Ithaca, N. Y., sectioning mature seeds.

Table No. 8—M. B. Thomas, Cornell University, Ithaca, N. Y., the sectioning of large pistils.

Table No. 9—L. D. McIntosh, Chicago, Ill., attachment for using a solar and oxy-hydrogen microscope, with direct or central and oblique illumination and illuminating and projecting opaque objects.

Table No. 10—F. L. James, Ph. D., M. D., St. Louis, Mo., mounting in glycerine, sharpening microtome knives.

Table No. 11—R. N. Reynolds, M. D., Detroit, Mich., (*a*) sectioning injected material; (*b*) mounting diatoms.

Table No. 12—Prof. Henry L. Tolman, Chicago, Ill., methods of preparing slides of blood.

Table No. 13—G. S. Woolman, Esq., New York, N. Y., homogeneous immersion object glass, direct and oblique illumination.

Table No. 14—J. A. Close, M. D., Summerfield, Ill., (*a*) injecting tree-frog with special apparatus; (*b*) dissecting with Stephenson's binocular; (*c*) living trichinæ in muscle of white rat; (*d*) White's improved life slide; (*e*) Close's tadpole slide; (*f*) Close's frog plate; (*g*) Close's balsam bottle.

At 4 o'clock a call was made for attendance in the assembly-room to listen to the reading of papers as follows:

Stuart H. Perry: Freshwater Rhizopods of Oakland County, Michigan.

Dr. M. D. Ewell: Two New Forms of Stage Micrometer. (See p 76.)

Accompanying this paper the Doctor showed a glass plate for photogravure work, 8 by 10 inches in area, ruled with parallel lines 100 to the inch, covering the entire surface and without a defect in the ruling of the whole surface—a triumph of skill and perfected method.

Dr. W. H. Seaman presented a drawing with descriptions of a new form of microscope.

Dr. Mary A. Spink read a paper upon a Microscopical Study of the Choroid Plexus.

The President: Dr. Spink is the first lady to read a paper before our Society upon a histological subject and I am sure we all congratulate her upon the success attendant upon her studies and the presentation of the results.

Mason B. Thomas then read a paper upon the Celloidin Method in Botany.

Professor Gage simply remarked that any one who has seen slides prepared by Mr. Thomas in the manner described needed no further evidence of the value of his methods.

Mr. Tolman: I had believed that dehydrating and hardening soft vegetable tissues could not be successfully accomplished, but the ocular demonstration is absolutely convincing. The process shall be given a trial at my house.

A paper on the Preparation and imbedding the Embryo of Chick, by Messrs. S. H. Gage and G. S. Hopkins, was read by the latter.

Professor Gage: Mr. Hopkins did all the work described in this paper except devising the glass tacks and their arrangement for use. Taking the set of these in his hand, the speaker detailed the advantages of this method of holding the prepared corks under the hardening liquid and made plain the improvement over the older ways.

After this the meeting adjourned.

T. J. BURRILL, *Secretary.*

THURSDAY EVENING, *August 14, 1890.*

This evening was devoted to the exhibition of microscopic objects at Princess Rink, a very large inclosure excellently adapted for the purpose. Under the direction of Dr. R. N. Reynolds, of Detroit, arrangements had been made for a magnificent display and for the accommodation of a great throng of spectators. There were 177 numbers printed in the catalogue of microscopes and objects, and this number was somewhat more than equaled upon the tables. Notwithstanding the elaborate preparations made for a great concourse of invited guests, many of the latter could not make the rounds of the instruments. Those who did get a chance to look lingered too long over the wonders of the world of little things, and the movement of the long files was too slow for the crowd of eager unfortunates outside of the envied succession. As is universally the case, the low-power exhibits were most attractive, and anything living disproportionally detained the observers. To the many who beheld the microscopic wonders for the first time the evening will be long remembered and the Society will gain by the pleasure thus afforded.

FRIDAY, *August 15, 1890.*

The Society convened at 10 o'clock, President Fell in the chair. The following-named persons, upon recommendation of the Executive Committee, were elected members: G. Carl Huber, M. D., Ann Arbor, Mich.; Wm. Gaylord, Sandusky, Ohio; John Kapp,

M. D., Ann Arbor, Mich.; Fred. H. Seymour, Detroit, Mich.; E. T. Hartley, Lincoln, Neb.; Isaac Douglas, M. D., D. D. S., Romeo, Mich.; F. Wilmot Brown, M. D., Detroit, Mich.; Wm. C. West, M. D., Monroe, Mich.

The first paper upon the program was on Fees of Experts with the Microscope, by C. M. Vorce, in his absence read by Dr. F. L. James.

The reading of this paper elicited much discussion among the members of the Society, both in and out of the meeting. No one could question the propriety of this discussion nor its importance to members of the Society, and as the paper does not appear in full the following brief abstract by the Secretary is here inserted. The author argues that all persons who become skilled in the use of the microscope are liable to be called upon because of such skill to make examinations and report conclusions as an expert. This should and does operate as an incentive to increased care and skill in microscopical work and therefore encourages microscopical research in full accord with the purposes of the Society.

For convenience of discussion let us divide the subject into subheads as follows:

"1. Should fees be charged for mere matters of opinion by one who does not follow the occupation of microscopical examination as a business or profession?

"2. By what rule should the fees to be charged by the expert in making examinations with the microscope and reporting thereon be estimated?

"3. What is the proper attitude of the expert in the matter of fees and in the character of witness, and what, if any, rule of ethics should govern him in the matter of making examinations and returning opinions?"

1. After noting the assumption that for one possessing scientific knowledge it tends to degrade or belittle his acquirements by accepting pay for his opinion, and that if paid he will be inclined to make such opinion favorable to the employer, the author replies that the giving of gratuitous opinions leads inevitably to hasty and less trustworthy results. The "curbstone opinions" of lawyers are cited as an example, and the conclusion is reached that an opinion of the scientific expert "should be formed only after careful study of the specific subject of inquiry, and in the light of past experience and previous knowledge in the same line, and the time spent in so doing should be as justly paid for as if spent in any other employment."

2. It is a vicious principle to be goverend in regard to the amount of the fee by the amount at stake or in proportion to the ability of the client to pay, The true basis of all compensation for services is upon the amount of time and labor necessarily spent by the employé. The rate of compensation varies, very properly, according to the experience, ability, etc., brought to the task; but the basis of compensation remains the same. All fees of experts should be, and largely are, based upon a per diem estimate, and this should be the rule in microscopical work.

3. Professional ethics requires of every one that he should not disclose any matters communicated to him in a confidential capacity, or which came to his knowledge by reason of his employment in any such manner. The expert may examine the same object for any number of people, but he must not intimate to any one the opinion given to another, or even the fact that he had done so. The expert witness is bound by his oath to state the truth and the whole truth just as much as any other witness, and if he states less than the whole truth by suppressing facts which militate against his opinion he escapes perjury by an exceedingly close margin indeed. He has earned his fee when the work is completed, no matter whether his opinion is good or ill for the employer, and all that can be asked of him is to state the honest results obtained.

From the experience of nearly a quarter of a century the author says the fee should invariably be paid in advance, and that for further service either a per diem charge or a gross sum should be stipulated. "Above all things, the expert should avoid enlisting as a partisan on the side of his employer, but should give his opinion regardless of who is hurt or helped by it, which, if he has been paid for his time, he can do without any anxiety."

Dr. James: I will say that my practice is in accordance with the suggestions of this paper. A fee is charged for service rendered based upon a per diem rate of twenty-five dollars.

Dr. Ewell: Your amount is too small. Should at least make it fifty dollars, and one hundred would be better appreciated. I would rather have one than two cases for the same money, and a high estimate of one's services will never be amiss. Would emphasize the pay-in-advance principle, and do not accept engagement by prosecuting attorney unless you know that he has the authority of the board of supervisors. Especially, however, if you are engaged on the defense, the payment in advance is good advice, as I have considerable reason to know.

Dr. James: After sitting back in his office some time with nothing to do, perhaps it may be considered worth while for one who desires employment to put his fee at some such sum as can be paid.

Dr. Ewell: Of course, I am speaking of those like the Doctor himself, who have passed their apprenticeship and have become recognized authorities in their specialties.

President Fell asked Vice-President Seaman to take the chair and spoke upon the subject as follows:

The grounds that the amount of interest involved should not have a bearing upon the fee does not appear to be followed by the legal profession if the many reports of enormous legal fees are to be relied upon, and in many cases it appears unjust that in expert work where a few hundred dollars are at stake there should be allowed as great a fee as when many thousands are involved. Then again, special arrangements to the effect that if the expert should succeed in obtaining certain results he would be well rewarded might prove a stimulus to renewed exertion. This presupposes that all experts are honest. Of course if the result of their labor would not stand the scrutiny of their legal opponents, it would not be of great value. The Doctor related a case somewhat in question as follows: Certain parties employed him to examine into a case which was admitted by their attorneys and themselves to be desperate. They agreed to pay the Doctor a handsome fee if he succeeded in establishing a favorable aspect to their case. He did so. The lawyers took advantage of his work; made contracts with their clients by which they obtained an enormous fee, considerably over ten thousand dollars, and they considered that the expert's fee should be nominal. Their opposing attorney stated that if the expert had refused to give his knowledge in the case the clients would have willingly paid a very much larger sum than he obtained after divulging it.

What he contended for was that circumstance should modify the fee of the expert as well as the fee of the members of the legal profession.

Dr. Ewell: Any conditional agreement based upon the results of a decision or verdict is contrary to public policy and void. A case was cited in which such contingent additional fee was agreed upon in the event of the success of the employer's side and though accruing according to stipulation was not paid and was brought to trial. The court decided the payment not legally authorized, because such stipulations tended to bias testimony and were contrary to good policy. The fee should be made large enough to begin with and then abide by the bargain.

Dr. Mercer related the story of a prominent attorney in his town with whom another prominent citizen and acquaintance chatted upon legal matters while walking together upon the street. Prominent citizen afterward carried a case to the courts and lost, then asked attorney how it happened that his opinion so radically differed from that of the court. In reply, the query was propounded as to the amount paid for said opinion. Also another, of a man from a neighboring village who went into a physician's office to consult about treatment and asked about cost of same. Physician, supposing the man to be poor and unaccustomed to large bills, named ten dollars. Poor man sought another physician, and for same thing had named one hundred dollars as fee; accepted latter, saying there were plenty of ten-dollar doctors in his own village. The inferences here are sufficiently plain.

Professor Tolman expressed himself as much pleased with the suggestions of the paper, remarking that he had charged up a considerable number of accounts to experience, and was, he hoped, the wiser in consequence; did not think a contingent fee wise or proper. Its tendency was bad. The best way is to make an original fee large enough for the proposed service and then agree upon a per diem allowance for further work; had not demanded pre-payment, but should have had less to charge to experience in that way.

There is the question of, who is an expert?

One answer is that an amateur does not, and an expert does, charge for his services. This certainly is not a good definition; but at best the distinction seems to be left with the man himself to make. The trouble with expert testimony is that the law makes no distinction in this respect. We ought in some way to have the term expert properly defined, and then an improvement might be made in this matter in the courts. Assuming now that an expert is one who makes it his business to professionally examine and to report upon matters in dispute within his province, is one who, after such examination, has given an unfavorable report, open to engagement on the other side? The case is different with a lawyer. The law does not require him to divulge secrets obtained in a professional way, but a witness is sworn to tell what he knows and all he knows. A lawyer may also engage upon the wrong side and, if he wins, public opinion supports him in his professional endeavors. The two are different also in this respect: No mercy is shown an expert witness if the decision seems to be against him. Upon the whole, it would seem proper that an expert may properly engage upon the opposite side after having rendered an adverse report, but that he ought not to be required to divulge any information obtained by reason of his first employment.

Dr. Detmers: Is an expert on the witness stand obliged to answer all questions put to him? Is he obliged to tell everything he happens to know upon a given subject?

Dr. Ewell: The law makes no difference between witnesses, whether experts or not; but no one is obliged to do any work, to seek out information, against his will.

At this point Dr. Manton asked leave to call attention to Grace Hospital, which was left out from the published list of public institutions worthy of a visit by members of the Society.

The Treasurer's report being called for was presented by C. C. Mellor, and also report on Spencer-Tolies fund.

The report of the committee on nomination of officers reported as follows:

NOMINATIONS FOR 1891.

President	Dr. F. L. James, St. Louis, Mo.
Vice-Presidents	Prof. E. W. Claypole, Akron, Ohio.
	Prof. M. D. Ewell, Chicago, Ill.
Secretary	Dr. W. H. Seaman, Washington, D. C.
Treasurer	C. C. Mellor, Pittsburgh, Pa.
Elected Members of Executive Committee	Dr. E. L. Shurley, Detroit, Mich.
	Dr. J. O. Stillson, Indianapolis, Ind.
	Dr. A. Clifford Mercer, Syracuse, N.Y.

C. C. Mellor: Mr. President, I wish to explain that the person who appeared to be the choice of the majority of committee for the office of Treasurer stated that he felt constrained to decline the nomination; not that he did not appreciate the honor and feel highly complimented by the confidence in him implied by the offer of the office, but simply because his many business engagements so fully occupied his time that it was difficult to attend properly to the duties of the Treasurer.

Another difficulty in the way was that the gentleman proposed was a member of the Nomination Committee, and it was very questionable propriety for him to be nominated by a committee of which he was a member; but there are precedents for this in the history of the Society. However, the long and the short of it was that the gentleman found himself in the hands of six bold and bad men—a sort of six-armed octopus—and there seemed to be no escape from their grrsp. The gentleman, like Davy Crockett's coon, came gracefully down the tree.

He insisted, however, that the duties of Custodian must be separated from that of the Treasurer, as has already been proposed.

On motion, the Secretary was instructed to cast the ballot of the Society for the persons named for the respective offices. This being done, they were declared duly elected.

Dr. Mercer moved that the President be authorized to appoint delegates to a celebration at Antwerp of the invention of the microscope. (See note, page 246.)

A paper upon the Identification of hair, by Dr. Ewell, was read by title and referred to Committee on Publication, and the same disposition was made of one by Dr. Howard N. Lyon on "A cover-glass support for mounts in solid media."

A discussion now took place upon the advisability of meeting at the same time and place the American Association for the Advancement of Science, opened by Dr. W. H. Seaman, who, in brief, said :

The members of this Society consist of two classes, viz., those who are amateurs or those who use the microscope for the love of it, and those who use it in their professional occupation. This Society was founded by the first class, but is increasingly attended by those belonging to the second class. In 1879, of 104 members eight were members of the A_3S, and last year, of 340 members 33 are members of the latter association. Perhaps it is true that the members of this second class are now doing the most work. Miss Booth, of Long Meadow, Mass., suggests that the time elapsing between the two meetings be shortened, and I am inclined to think it would be desirable for us to meet at or near the same place with this large association and upon Thursday instead of Tuesday of the week preceding, permitting our session to extend so as to include Monday instead of Friday, as at present.

Professor Gage : I am also a member of the A_3S, and usually attend the meetings, but I do not think it would at all answer for us to meet at the same time and place with that association. We should simply be a tail to it and nothing else.

Dr. Detmers : Yes, sir ; and a microscopic tail at that. We should keep ourselves entirely independent ; pay no attention to others and suit ourselves. It seems impossible to get railroad reductions available for both meetings when held near together, since tickets can be bought only three days before the first meeting and good only three days afterward. I can now return from Detroit to Columbus at one-third rate, and may in the same way return from Indianapolis, if I choose to attend, at same reduction.

The President: May it not be as well to meet near the American Medical Association?

Dr. James: That association has a large membership and the hotels are crowded. It is not wise to attempt to meet at same place and time with these larger societies, but if we can meet near the Association for Advancement of Science it would accommodate several of our members.

Dr. Seaman: It might be better to meet near the A_3S, and upon Wednesday rather than Tuesday.

Dr. Ewell: We ought to have more time. Our sessions are not long enough; should have more time for discussion; ought to have two more days.

Dr. Mercer: In our discussions we wander away from the topic. What we want is closer attention to the matter before us, and less time would suffice.

The President: We have been getting along in good order, finishing our program for each session and having good discussions also.

Professor Claypole: It will be a great convenience for many of us to meet near the Association for the Advancement of Science.

This appeared to be the opinion of the majority, but upon vote the time and place of meeting was left to the Executive Committee.

It was moved and carried that the Treasurer be authorized to appoint an assistant, to act as Custodian.

The Committee on the Uniformity of Tube Lengths presented the report found on page 247.

Dr. Mercer: We owe a great deal to the original mover in this matter, and now should like to secure still further services of same kind. I move that Professor Gage be appointed a committee to correspond with the opticians of the world upon the subject and to report progress at our next meeting.

Motion prevailed.

Dr. Detmers: Mr. President, there is another thing to which I wish to call attention. We now know that Zeiss uses fluorspar in the construction of his apochromatic objectives. Yesterday I happened to be in Cleveland, Ohio, and called upon Herbert R. Spencer. When this matter of the use of fluorspar in objectives was mentioned he showed me, in the handwriting of his father, Charles A. Spencer, several formulæ for objectives with lenses of this substance. Here, then, we have another instance of American invention of which the credit has gone to the other side of the ocean. The full facts in this connection can be ascertained, and I move that the Secretary be instructed to correspond with Mr. H. R. Spencer and to publish the results in connection with the proceedings of this meeting.

Carried.

Dr. Deck: Did Spencer actually construct objectives containing fluorspar?

Dr. Detmers: Yes, sir; I have seen the records in the regular books kept at the time for the purpose of entering the objectives under their regular order of manufacture.

The next paper was by Mrs. S. P. Gage on the "Form and endings of striated muscular fibers," read by Professor Gage.

This was followed by the "Comparative structure of the external and middle ear of the cat," by Thomas B. Spence; also read by Professor Gage.

The latter remarked that this last was a thesis presented and accepted for a bachelor's degree in Cornell University, and he had urged that it be brought here because of its excellence. It was one of the best pieces of work of the kind that had ever come to his knowledge, and he should be glad to have others see it.

Dr. Mercer: Perhaps it is not the thing to formally propose a vote of thanks to Professor Gage and to Cornell University, but I do want to express the high appreciation we all have of the excellent work done at that great institution and contributed to the program of this meeting, especially in the department of which Professor Gage is the head.

Dr. Detmers moved that the Committee on a Proposed Journal for the Society be discharged.

Motion prevailed.

The proposed changes in the Constitution and By-Laws in regard to the publication of proceedings, custodian, and associate members were read and referred to the Committee on New Constitution.

The President appointed a Committee on Final Resolutions, as follows: W. H. Seaman, H. L. Tolman, D. L. McIntosh, E. W. Claypole, and C. C. Mellor.

Adjourned to meet on the boat Fortune, on Detroit river, at 2 o'clock.

T. J. BURRILL, *Secretary.*

AFTERNOON SESSION AND EXCURSION.

At 2 o'clock the members and some invited friends assembled on board the steamer Fortune and enjoyed a magnificent trip on one of the finest waters in America, while at the same time the hours passed pleasantly in social intercourse and in further making acquaintance one with another. Upon the special invitation of Parke, Davis & Co.* the entire body landed at their wharf and were shown through their immense laboratories, while the various processes in the manufacture and preparation of drugs and medicines were in active operation. No description of this great business can here be given, but the members of the Society expressed by words and acts their intense interest in what they saw and their obligation to the proprietors and employés for the attentions shown them. Returning to the boat, they were further delighted by the appearance of these same employés in a new rôle, viz., attendants in serving a most enjoyable and heartily appreciated collation, which microscopists have never been known to refuse despite the revelations of high-power objectives.

Finally President Fell again called the Society to order, and, after some remarks upon the success attendant upon the Detroit meeting and the pleasure of the occasion, called for the report of the Committee on Final Resolutions. Instead of the formal statements usually presented in writing the report, took rather the shape of toasts and responses, the latter by various persons called upon for the purpose. This was a most agreeable departure from ordinary

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*WEDNESDAY, *June 4, 1890.*

Prof. T. J. BURRILL, *Champaign, Illinois.*

DEAR SIR: Learning that the annual meeting of the American Society of Microscopists will be held in this city about August 12th, and thinking perhaps that while here an inspection of our laboratory might be of interest, it affords us pleasure to invite you to visit it for that purpose. We cordially extend to you this invitation at the kind suggeston of Mr. Henry W. Fuller, of New York, a member of your body.

As chemists and pharmacists, we have long since learned the great value of the microscope and we fully recognize the benefits that we derive from the researches of the microscopist himself. Feeling thus an obligation, we would like in some measure to reciprocate it by affording you such interesting information as a visit to our laboratory may impart. We can at least assure you of a hearty welcome, and at the same time we shall endeavor to make you feel that your time has not been illspent should you honor us with your presence.

Hoping to have the pleasure of your acceptance, we are,

Very truly your friends,

PARKE, DAVIS & CO.

usage, and gave an opportunity to the gifted ones to display their wit and ready repartee. In this way thanks were offered to the local committee and citizens of Detroït, responded to by Dr. E. L. Shurley, of Detroit; to the daily press of Detroit, responded to by Edward Bausch, of Rochester, N. Y.; to the railroad traffic associations, responded to by Dr. R. N. Reynolds, and to Parke, Davis & Co., responded to by Mr. W. Anderson, of that firm.

After a vote of thanks to the retiring President and Secretary for their labors in behalf of the meeting, the former said there must come an end to all things except holding office by himself, for he had attended the first meeting of the Society and had been in office ever since. Now, as past President, he should be a member of the Executive Committee. We all desire in this world to have a good time, but really only those succeed who have hard work to do and accomplish it with willing and resolute hands; hoped that officers and members would all work faithfully for the good of the Society, not waiting one for another, but each making himself foremost in every good way. Now you have elected a better man than myself as president, and all that is left for me to do is to resign my place to him.

President-elect Dr. F. L. James, of St. Louis, then stepped forward and accepted the honor thus conferred upon him, though he feared the Society had made a mistake in electing a man who could not conduct its affairs to the high degree of success heretofore attained. He was well aware of the labor required on the part of the chief officers of the Society and entered upon the duties of President with his eyes open upon that point, yet would promise to do the best possible under the circumstances, and would strive to be faithful in the discharge of the duties devolving upon him.

There being a few moments to spare before reaching the wharf, Dr. Seaman spoke to "the good of the order," after which the Society adjourned to the time and place to be selected by Executive Committee in 1891.

T. J. BURRILL, *Secretary.*

NOTE.—The 300th Jubilee anniversary of the discovery of the microscope. See Journal Royal Microscopical Society, August, 1890, p. 522.

W. H. S.

Discussion on Photomicrography as Corrected by Dr. Mercer.

Dr. Mercer could not agree with Governor Cox in holding that an eye-piece should always be used with the objective to get the best results in photomicrography. He called attention to the fact that in the ordinary use of the microscope a virtual image is seen; and, further, that the whole instrument is corrected to make that virtual image as perfect as possible for rays of light taking mathematically defined paths through the various lenses. In photomicrography with the whole instrument, not a virtual but a real image is formed by rays taking not the same but quite different paths through both the objective and eye-piece. Now, for example, the rays incident on the eye-lens come from an aerial object beyond its principal focus, while in the first instance rays are from an aerial object within the principal focus of that lens. The eye-lens is not achromatic, but in the first instance its aberrations are corrected by its relative position. In the second instance the radical change in the paths of the rays allows both chromatic and spherical aberrations to vitiate still farther the already faulty image formed by the objective working at the disadvantage of a change in position (at a greater distance from the object than that for which has been so nicely corrected). To get the best results in photomicrography the objective should be left in its ascertained best position and collar correction and the "ten-inch" back focus extended to the required distance by means of a specially corrected concave lens (the Woodward method) or the ordinary eye-piece should be replaced by a specially corrected projecting eye-piece (the Abbe method). The Huyghenian eye-piece is corrected for a radically different purpose and is, therefore, at least theoretically, quite out of place as a projecting lens.

BUFFALO, N. Y., *Jan. 27th, 1891.*

Prof. T. J. BURRILL.

My Dear Sir: Your favor* of 21st inst. came to hand this morning. Since coming to Buffalo my time has been so fully occupied that I had, for a long time, forgotten entirely the subject mentioned in your letter. As the time is so limited, and having no knowledge of how much space you had intended to give to the subject in the forthcoming publication of the proceedings of the A. S. M., I have thought best to state to you, as briefly as possible, the facts, leaving it to you to arrange them in proper form for publication, with whatever comments you see fit to make.

During the summer of 1860 Dr. Rufus King Brown, who was, at that time, a resident of Brooklyn, N. Y., visited my father at Canastota, and during his stay there my father made for him a 1-8 objective, which was considered by all who saw it to be the best ever made up to that date. Although but a young boy at the time, I was greatly interested in my father's work, and knew pretty well what was going on—hearing a great deal of talk, and remember well Dr. Brown's high praise of the performance of the objective; but of course knew nothing of its construction until some years later. Some years later, after Mr. Tolles had removed his establishment to Boston, Dr. Brown became a resident of that city, and showed the objective to Tolles, who praised its performance very highly.

The angular aperture of the objective was 175°. One of the systems contained fluorspar, and it is on record in the formula that it was remarkably perfect in its corrections for both figure and color, with both oblique and central illumination. In the years 1864 and 1865 I made lenses for quite a number of objectives, mostly 1-4 in., containing at least one lens of fluorspar, and having apertures from 170° to 176°; but of course with very short working focus. In all these objectives, as well as in the one made for Dr. Brown, the spar lenses were cemented between others, as owing to its softness and liability to become scratched it was not considered safe to leave it in an exposed position. About the time that immersion lenses came into general favor in this country the use of the spar was abandoned, owing to the difficulty experienced in procuring that which was free from fractures or seams. When used in the 1-4 I dreaded making them more than all others in the objectives, often having to throw them away, owing to their having such defects. Shortly after our removal from Canastota to Geneva I made a 1-12 water im., containing one spar lens; but that is the only one since I was a boy. When Dr. H. J. Detmers, of Columbus, O., visited me at Cleveland (about the time of the meeting of the A. S. M. at Detroit) I showed to him the old book containing the records of all these objectives as kept by my father, and among them the 1–8, marked R. K. B., also 1–8, containing a spar lens; which was made as far back as 1851, about the time my father begun making objectives of large angle, showing how early in the fight he appreciated the valuable optical qualities of fluorspar in the construction of objectives. The angular aperture of this 1–8 was not given, but I can readily see that it could not have been less than 160°.

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*This letter was received by me Jan. 31, 1891, after the preceding pages were in press. Professor Burrill informed me by accompanying letter that he had sent a request to Mr. Spencer for information as to the facts about fluorspar objectives.

W. H. S.

These are, I think, all the facts necessary, but I may have made too much of it. It has been hurriedly written off and is not in proper shape for publication, but you can cull out the facts and arrange them properly for publication in the forthcoming proceedings. I shall esteem it a great favor if you will do so, as I look upon the facts as of value in connection with the history of the microscope.

I trust you will pardon the use of the pencil, for I have been anxious to send this by the first mail. Please let me know if it reaches you in time.

Very truly yours,

H. R. Spencer.

REPORT OF THE COMMITTEE OF THE AMERICAN SOCIETY OF MICROSCOPISTS ON UNIFORMITY OF TUBE-LENGTH.

Believing in the desirability of a uniform tube-length for microscopes, we unanimously recommend :

1. That the parts of the microscope included in the tube-length should be the same by all opticians, and that the parts included should be those between the upper end of the tube where the ocular is inserted and the lower end of the tube where the objective is inserted.

2. That the actual extent of tube-length as defined in section 1—Be, for the short or continental tube, 160 mm., or 6.3 inches, and 8½ inches, or 216 mm., for the long tube, and that the draw-tube of the microscope possess two special marks indicating these standard lengths.

3. That oculars be made par-focal, and that the par-focal plane be coincident with that of the upper end of the tube.

4. That the mounting of all objectives of ¼ inch and shorter focus should be such as to bring the optical center of the objective 1½ inches below the shoulder, and that all objectives be marked with the tube-length for which they are corrected.

5. That non-adjustable objectives be corrected for cover glass from $\frac{15}{100}$ to $\frac{20}{100}$ mm. ($\frac{1}{170}$ to $\frac{1}{130}$ inch) in thickness.

These recommendations give a distance of 10 inches (254 mm.) between the par-focal plane of the ocular and the optical center of the objective for the long tube, and are essentially in accord with the actual practice of opticians.

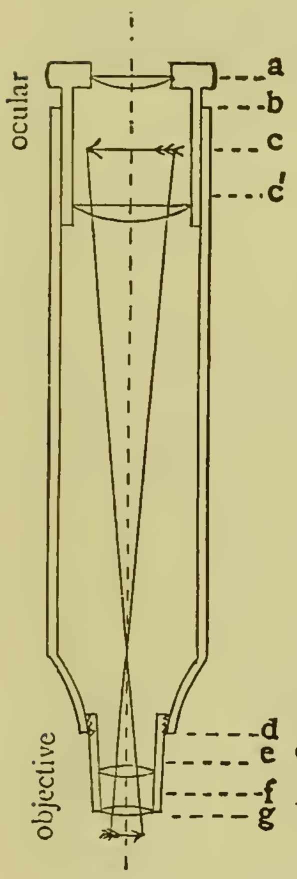
At the request of the committee, a joint conference was held with the opticians belonging to the Society and present at the meeting. They expressed their belief in the entire practicability of the above recommendations and a willingness to adopt them.

(Signed)

Simon H. Gage.
A. Clifford Mercer.
Charles E. Barr..

Tables and Cut Reproduced From the Proceedings (1887, pp. 168-172) to Illustrate the Lack of Uniform Standards of Tube-Length and Thickness of Cover Glass for Which Non-Adjustable Objectives are Corrected, and Hence the Desirability of Some Action Like the Above Recommendations Looking Toward Uniformity.

Length in Millimeters and Parts Included in "Tube-Length" by Various Opticians.



Pts. included in "Tube-length." See Diagram.		"Tube-length" in Millimeters.
a-d	Grunow, New York	203 mm.
	Nachet et Fils, Paris	146 or 200 mm.
	Powell and Lealand, London	254 mm.
	C. Reichert, Vienna	160 to 180 mm.
	W. Wales, New York	254 mm.
b-d	Bausch & Lomb Opt. Co., Rochester	216 mm.
	Bézu, Hausser et Cie, Paris*	220 mm.
	Klönne und Müller, Berlin	160–180 or 254 mm.
	W. & H. Seibert, Wetzlar	190 mm.
	Swift & Son, London	165 to 228½ mm.
	C. Zeiss, Jena	160 or 250 mm.
a-g	Gundlach Optical Co., Rochester	254 mm.
a-g	R. Winkel Göttingen	220 mm.
c-d	Ross & Co., London	254 mm.
c-e	R. & J. Beck, London	254 mm.
c-g	H. R. Spencer & Co., Geneva, N. Y.	254 mm.
c-f	J. Green, Brooklyn†	254 mm.
c′-e	E. Leitz, Wetzlar	125–180 mm.
	For oil immersions	160 mm.

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* Successors to Hartnack.
† Successor to Tolles.

Thickness of Cover Glass for Which Non-adjustable Objectives are Corrected by Various Opticians.

$\frac{25}{100}$mm.	J. Green, Brooklyn.
	J. Grunow, New York.
	Powell and Lealand, London.
	H. R. Spencer & Co., Geneva, N. Y.
	W. Wales, New York.
$\frac{18}{100}$mm.	Klönne und Müller, Berlin.
$\frac{17}{100}$mm.	E. Leitz, Wetzlar (when tube 160–170 mm.).
$\frac{17}{100}$mm.	R. Winkel Göttingen, Germany.
$\frac{16-25}{100}$mm.	Ross & Co., London.
$\frac{16}{100}$mm.	Bausch & Lomb Optical Co., Rochester.
$\frac{15-20}{100}$mm.	C. Zeiss, Jena ($\frac{16}{100}$mm. for apochromatic oil immersions).
$\frac{15-18}{100}$mm.	C. Reichert, Vienna.
$\frac{15}{100}$mm.	Gundlach Optical Co., Rochester.
	W. & H. Siebert, Wetzlar.
	R. & J. Beck, London.
$\frac{12-17}{100}$mm.	J. Zentmayer, Philadelphia.
$\frac{10-12\frac{1}{2}}{100}$mm.	Nachet et Fils, Paris.
	Bézu, Hausser et Cie, Paris.
$\frac{10}{100}$mm.	Swift and Son, London.

REPORT OF CONDITION OF TOLLES-SPENCER FUND AUGUST 15. 1890.

Cost of one share of stock in the Perpetual Savings and Building Association of Urbana, Ohio, purchased by former Treasurer, D. S. Mosgrove	$200 25
Cash on hand	53 61
Total paid subscriptions and accrued interest	$253 86

(Signed) C. C. MELLOR, *Treasurer.*

REPORT OF TREASURER C. C. MELLOR.

RECEIPTS.

Cash in hand August, 1889	$26 62
Received dues for 1887	4 00
" " " 1888	8 00
" " " 1889	40 00
" " " 1890	497 00
" " " 1891	18 00
Received admission fees 1890	60 00
" " " 1891	9 00
Advertisements in Proceedings, 1889	49 00
Sale of Proceedings	66 86
Total income	$778 48

EXPENDITURES.

For printing, binding, and mailing Proceedings	$481 22
Postage, freight, and other expenses	118 42
Cash on hand	178 84
	$778 48

We, the subscribers, hereby certify we have examined the foregoing account of C. C. Mellor, Treasurer, for the year from August 20, 1889, to August 11, 1890, and have found the same correct, with proper vouchers for the various expenditures.

Signed by— WM. H. SEAMAN.
F. W. KUHNE.

DETROIT, *August* 12, 1890.

CONSTITUTION AND BY-LAWS

OF THE

AMERICAN SOCIETY OF MICROSCOPISTS.

CONSTITUTION.

Article I.—This organization shall be called the "American Society of Microscopists." Its object shall be the encouragement of microscopical research.

Article II.—Any person interested in microscopical science may become a member of the Society upon recommendation, in writing, by the members, nomination by the Executive Committee, and election by a majority of the members of the Society present at any regular session of the Society. Honorary members may also be elected by the Society on nomination by the Executive Committee.

Article III.—The officers of this Society shall consist of a President and two Vice-Presidents, who shall hold their office for one year, or until their successors are elected and qualified, and shall be ineligible for re-election for two years after the expiration of their terms of office ; together with a Secretary and a Treasurer, who shall be elected for three years and be eligible for re-election.

Article IV.—The duties of the officers shall be the same as in other similar organizations ; in addition to which it shall be the duty of the President to deliver an address during the meeting at which he presides, of the Vice-President to audit the Treasurer's accounts, of the Treasurer to act as custodian of the property of the Society, and of the Secretary, with the advice and assistance of the President and Treasurer, to act as Publishing Committee and to edit and publish the Proceedings of this Society.

Article V.—There shall be an Executive Committee, consisting of the officers of the Society, the past Presidents, and three members elected by the Society.

ARTICLE VI.—It shall be the duty of the Executive Committee to fix the time and place of meeting of the Society, arrange the business, manage the general affairs of the Society, and superintend the publication of the Proceedings.

ARTICLE VII.—The election of officers shall be held on the morning of the last day of each meeting. Their terms of office shall commence at the close of the meeting at which they are elected.

ARTICLE VIII.—Candidates for office shall be nominated by a committee consisting of seven members of the Society. The committee shall be elected by a plurality vote by ballot after free nominations.

ARTICLE IX.—The initiation fee shall be three dollars, and the dues shall be two dollars, paid in advance annually. No applicant shall be considered a member until he has paid his dues; and any member failing to pay his dues for two consecutive years, and after due written notification from the Treasurer, shall forfeit his membership.

ARTICLE X.—Amendments to the Constitution may be made by a two-thirds vote of all members present at any annual meeting, after having been proposed at the preceding annual meeting.

BY-LAWS.

First—Papers read before the Society shall be acted upon by the Executive Committee at their next session and the question of their publication in the Proceedings decided, in order that notice of such publication may be given to the authors of papers by the close of the meeting.

Second—All papers accepted for publication shall be referred to a Publishing Committee, consisting of the Secretary as chairman, with the President and the Treasurer as advisory members.

Third—The manuscript and drawings of any paper accepted for publication shall be filed with the Secretary by the middle of September next succeeding the meeting at which such paper was read.

Fourth—It shall be the duty of the Publishing Committee to issue the volume of Proceedings not later than the first of January next succeeding each meeting.

Fifth—An edition of six hundred copies of the Proceedings shall be published each year; in addition to which each author of papers shall be furnished with twenty-five copies of his paper (with accompanying plates) gratis, and as many more, at the cost of publishing, as he may request at the time that his manuscript is sent to the Secretary.

Sixth—Papers accepted for publication by the Society may be published in any reputable journal after they have been read before the Society, provided due acknowledgments are made of the fact that they are from the Proceedings of the American Society of Microscopists, and that the original manuscript shall be left on file with the Secretary of the Society.

Seventh—Any member failing to pay his dues for three consecutive years shall be dropped from the roll of members, with the privilege of reinstatement at any time by payment of all arrears, and the annual Proceedings shall not be sent to any member whose dues are unpaid.

Eighth—All motions or resolutions relating to the business of the Society shall be referred for consideration to the Executive Committee before discussion and final action of the Society.

MEMBERS.

Lists of members are published in the Proceedings every year except 1887. The figures denote the year of the members' election, except '78, which marks an original member. The Proceedings are not sent to members in arrears, and two years' arrearage forfeits membership. (See Art. IX Constitution, and By-law VII.)

Aberdein, Robert, M.D., F.R.M.S., '82	Syracuse, N. Y.
Allen, W. L., M.D., '86	Greenbush, N. Y.
Alling, Chas. E., F.R.M.S., '84	Rochester, N. Y.
Alter, M. H., M.D., '86	Los Angeles, Cal.
Atwood, E. S., '79	242 South St., New York City.
Atwood, H. F., F.R.M.S., '78	Rochester, N. Y.
Ayres, Morgan W., M.D., '87	Upper Mont Clair, N. J.
Baker, S. Winsor, '86	503 E. 2d St., Jamestown, N. Y.
Barr, Prof. Chas. E., '90	301 Oswego St , Albion, Mich.
Baker, J. E., M.D., '89	Wyoming, Ohio.
Blackham, Geo. E., M.D., F.R.M.S., '78	Dunkirk, N. Y.
Barclay, Alexander, '83	St. Paul, Minn.
Barnes, Edwin R., M.D., '86	1258 West Ave., Buffalo, N. Y.
Barrett, W. C., M.D., D.D.S., '79	208 Franklin St., Buffalo, N. Y.
Bartges, Arthur F., '90	Akron, Ohio.
Bahrenburg, W. N., M.D., '89	99 Washington St., St. Louis, Mo.
Bassett, Chas. H., '82	504 Washington St., Boston, Mass.
Bausch, Edward, '78	179 N. St. Paul St., Rochester, N. Y.
Bausch, George R., '80	20 Arcade St., Rochester, N. Y.
Bausch, Henry, '86	Rochester, N. Y.
Bausch, William, '88	Rochester, N. Y.
Bering, J. Edward, '88	Decatur, Ill.
Blesch, P. E., M.D., '88	198 S. 3d St., Columbus, O.
Bleile, A. M., M.D., '81	277 S. 4th St., Columbus, O.
Bigelow, W. S., '90	Buffalo, N. Y.
Brittain, W. C., '89	Detroit, Mich.
Boyce, James C., '86	91 Water St., Pittsburgh, Pa.
Booth, Augustine Rue, M.D., '87	622 Crockett St., Shreveport, La.
Booth, Mary A., F.R.M.S., '82	Longmeadow, Mass.
Book, James Burgess, M.D., '90	39 Fort St., Detroit, Mich.
*Brayton, Forrest W., M.D.	Carey, O.
Bridge, John,' 90	261 Crawford St., Detroit, Mich.
Brown, F. Wilmot, M.D., '90	Detroit, Mich.

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* Deceased.

Briggs, Chas. M., M.D., '84 -------- Fairport, N. Y.
Briggs, Ira E., '86 -------- Erie, Pa.
Brown, Albert P., Ph.D., '79 -------- 5th and Federal St., Camden, N. J.
Brown, Robert, '85 -------- Observatory Place, New Haven, Conn.
Brown, C. W., '87 -------- 211 W. 122d St., New York City.
Brode, H. S., '89 -------- Champaign, Ill.
Bulloch, W. H., F.R.M.S., '78 -------- Washington, D. C.
Burrill, T. J., Ph.D., F.R.M.S., '78 -------- Champaign, Ill.
Bull, James, '88 -------- Hanging Rock, O.
Buttolph, H. T., C.E., '80 -------- 13 City Hall, Buffalo, N. Y.
Bruns, Henry D., MD., '87 -------- 24 Baronne St., New Orleans, La.

Carter, Jno. E., '86 -------- Germantown, Pa.
Campbell, D. P., M.D., '88 -------- Green Springs, O.
Cartwright, Oscar E., '90 -------- 721 Macomb St., Detroit, Mich.
Cassatt, M., M.D., '86 -------- 313 Elm St., Cincinnati, O.
Calvin, S., Ph.D., '89 -------- Iowa City, Iowa.
Chamberlain, H. B., '84 -------- Denver, Col.
Chapman, L. P., '86 -------- Oberlin, O.
Clapp, Geo. H., '86 -------- 95 5th Ave., Pittsburgh, Pa.
Clapp, W. A., M.D., '80 -------- New Albany, Ind.
Clark, Frank M., '86 -------- Jamestown, N. Y.
Clarke, James F., '83 -------- 3434 Sansom St., Philadelphia, Pa.
Claypole, Edward W., B.Sc., F.G.S., '86 -------- Akron, O.
Craig, James, Jr, '83 -------- St. Joseph, Mo.
Cheeseman, E. L., '88 -------- Knowlesville, N. Y.
Cherry, James B., '88 -------- Pittsburgh, Pa.
Chester, Albert H., A.M., '88 -------- Hamilton College, Clinton, N. Y.
Cleland, James, Jr., M.D., '88 -------- 31 State St., Detroit, Mich.
Chittick, Wm. R., M.D., '90 -------- 83 Lafayette Place, Detroit, Mich.
Cobb, Chas. N., A. M., '86 -------- State Normal School, Oneonta, N. Y.
Colwell, Frank F., '82 -------- Urbana, O.
Close, James Alex., M.B., F.R.M.S., L.R C.P. (Edin.), L.R.C.S. (Edin.), '90 -------- Summerfield, Ill.
Coon, H. C., A.M., M.D., '82 -------- Alfred University, Alfred Centre, N. Y.
Coope, A. F., M.D., '86 -------- Oil City, Pa.
Couch, Francis G., '86 -------- 846 Broadway, New York City.
Coutant, E. S., M.A., '86 -------- Hawks Park, Fla.
Cox, Charles F., F.R.M.S., '85 -------- Grand Central Depot, New York City.
Cox, Jacob D., LL.D., F.R.M.S., '82 -------- Cincinnati, O.
Curtis, Lester, M.D., F.R.M.S., '79 -------- 35 University Place, Chicago, Ill.
Cutter, John A., M.D., B.Sc., F.S.Sc., '90 -------- 1730 Broadway, "The Ariston," New York City.
Cutter, Ephraim, M.D., '82 -------- 55th St. and Broadway, New York City.

Danforth, Geo. F., '86 -------- Jamestown, N. Y.
Daniels, C. M., M.D., '83 -------- 868 Main St., Buffalo, N. Y.
Davis, Fielding L., M.D., '83 -------- 209 Locust St., Evansville, Ind.

Davis, W. Z., '86 ---- Marion, O.
Davis, Frank, '90 ---- 118 W. Main St., Belleville, Ill.
Deck, Lyman L , M.D., '90 ---- Salamanca, N. Y.
Dennis, S. W., M.D., F.R.M.S., '78 ---- University of California, San Francisco, Cal.
Detmers, H. J., M.V.D., F.R.M.S., '81 -- State University, Columbus, O.
Detmers, Miss Freda, '87 ---- Columbus, O.
DePuy, Hiram, M.D., D.D.S., '87 ---- 72 Wylie St., Pittsburgh, Pa.
Drescher, Wm. E., '87 ---- Rochester, N. Y.
Dobson, Jennie M., M.D., '86 ---- Muskegon, Mich.
Doubleday, Henry W., '90 ---- 715 H St., Washington, D. C.
Dods, A. Wilson, '90 ---- Fredonia, N. Y.
Douglas, Isaac, M.D., '90 ---- Romeo, Mich.
Duffield, George, M.D., '84 ---- 480 Woodward Ave., Detroit, Mich.

Eastman, L. M., M.D., F.R.M.S., '82 - 349 Lexington St., Baltimore, Md.
Early, Lewis M., M.D., '87 ---- 865 E. Long St., Columbus, O.
Eddy, Mrs. J. C., '87 ---- "The Lincoln," Cleveland, O.
Eichberg, Jos., M.D., '87 ---- 371 W. 7th St., Cincinnati, O.
Elsner, John, M.D., '83 ---- Denver, Col.
Elwell, A. T., '89 ---- Council Bluffs, Iowa.
Entrikin, F. W., M.D., '87 ---- Findlay, O.
Erker, A. P., '80 ---- 617 Olive St., St. Louis, Mo.
Ewell, M. D., LL.D., M.D., F.R.M.S., '85 -- 97 Clark St., Chicago, Ill.

Farnham, Geo. L., A.M., '80 ---- Peru, Neb.
Faris, Chas. C., '90 ---- 2853 Arsenal St., St. Louis, Mo.
Francis, Mark, D.V.M., '87 -- A. and M. College, College Station, Texas.
Fraser, Anson S., M.D., '89 ---- Sarnia, Ont.
Feiel, Adolph, M. D., '81 ---- Columbus, O.
Fell, George E., M.D., F.R.M.S., '78 ---- 71 Niagara St., Buffalo, N. Y.
Fellows, Chas. S., F.R.M.S., '83 ---- Minneapolis, Minn.
Flexner, Simon, M.D., '88 ---- Louisville, Ky.
French, Gale, M.D., '86 ---- 511 Penn Ave., Pittsburgh, Pa.
French, S. H., M.D., '82 ---- 40 Church St., Amsterdam, N. Y.
Fields, A. G., M.D., '82 ---- Des Moines, Iowa.
Fish, Pierre A., '90 ---- Ithaca, N. Y.
Flood, T. L., D.D., '86 ---- Meadville, Pa.
Ford, D. R., D.D., '81 ---- Female College, Elmira, N. Y.
Fuller, Chas. G., M.D., F.R.M.S., '81 ---- 38 Music Hall, Chicago, Ill.
Fuller, Henry W., F.R.M.S., '82 ---- New Rochelle, N. Y.

Gaertner, F., M.D., '87 ---- 3613 Penn Ave., Pittsburgh, Pa.
Gage, Simon H., B. Sc., '82 ---- Cornell University, Ithaca, N. Y.
Gage, Mrs. Susannah S. Phelps, '87 ---- Ithaca, N. Y.
Gardner, Wm. C., '87 ---- P. O. box 2062, New York City.
Gaylord, Wm., '90 ---- Sandusky, O.

Garrettson, J. G., '89 ---- 379 Michigan Ave., Buffalo, N. Y.
Gawne, A. J., M.D., '89 ---- Sandusky, O.
Geddings, H. D., M.D., Surgeon U. S. Navy, '90 ---- Key West, Fla.
Gleason, S. O., M.D., '80 ---- Elmira, N. Y.
Green, Caleb, M.D., '83 ---- Homer, N. Y.
Greenleaf, I. T., M.D., '84 ---- Oswego, Tioga Co., N. Y.
Gruhlke, A. C., '90 ---- Waterloo, Ind.
Gifford, Herman L., '89 ---- Jamestown, N. Y.
Girling, Rob't N., '83 ---- Wash'n and Prytania aves., New Orleans, La.
Griffis, H. L., A.M., '84 ---- New Paltz, N. Y.
Griffith, E. H., A.M., Hon., F.R.M.S., '78 ---- Fairport, N. Y.
Griffith, J. D., M.D., '87 ---- 908 Wyandotte St., Kansas City, Mo.
Gundlach, Ernst, '79 ---- Rochester, N. Y.

Haag, D. E., M.D., F.R.M.S., '86 ---- Liberty Center, O.
Hall, Edward D., '89 ---- Bristol, R. I.
Harding, Lawrence A., ,90 ---- 112 Lincoln Ave., Fergus Falls, Minn.
Hartigan, James W., A.M., MD., '90 ---- Morgantown, W. Va.
Hazzard, T. L., M.D., '90 ---- 56 Montgomery Ave., Allegheny, Pa.
Hartley, Prof. E. T., '90 ---- Sup't City Schools, Lincoln, Neb.
Hanaman, C. E., F.R.M.S.. '79 ---- 103 1st St., Troy, N. Y.
Hanks, Henry G., '86 ---- 718 Montgomery St., San Francisco, Cal.
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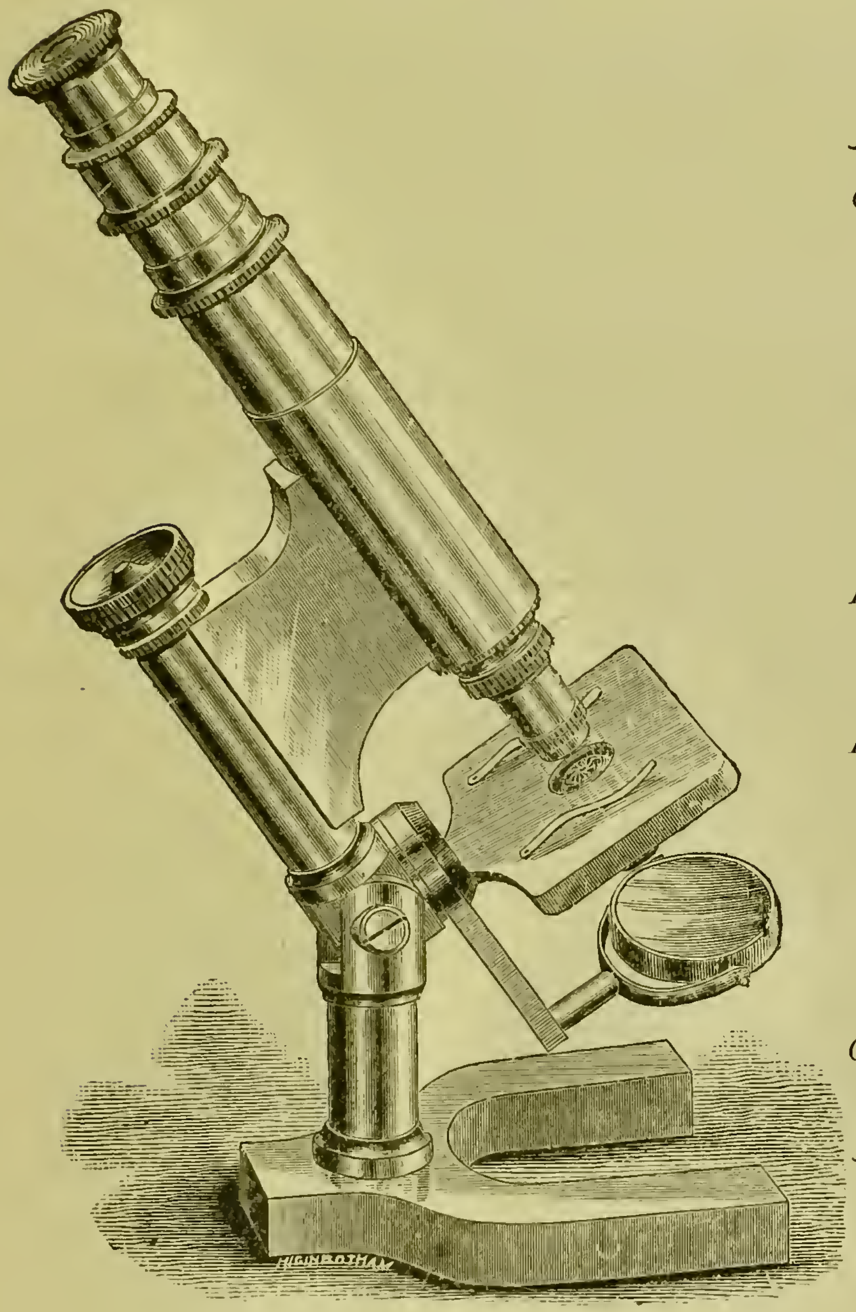

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The next Annual Meeting of the American Society of Microscopists will be held in Washington, D. C., August 11, 1891, the week preceding the meeting of the American Association for the Advancement of Science. Announcements will be made by special circular.

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